Vol. XXII, No. 1. PSYCHOLOGICAL REVIEW PUBLICATIONS January, 1915

THE Psychological Review

CONTENTS

PUBLISHED BI-MONTHLY BY

PSYCHOLOGICAL REVIEW COMPANY

41 NORTH QUEEN ST., LANCASTER, PA.

AND PRINCETON, N. J.

Entered as second-class matter July 13, 1897, at the post-office at Lancaster, Pa., under Act of Congress of March 3, 1879

Psychological Review Publications

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Annual Subscription to Review and Bulletin, $5.00 (Canada, $5.15, Postal Union, $5.30); Review, Bulletin, and Index, $5.85 (Canada, $6.00, Postal Union, $6.15); Bulletin, Alone, $2.75 (Canada, $2.85, Postal Union, $2.95).

Current Numbers of the Review, 50c.; of the Bulletin, 25c. (special issues 40c.); of the Index, $1.

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PSYCHOLOGICAL REVIEW COMPANY
Princeton, New Jersey

VOL. XXII. No. 1 January, 1915

THE PSYCHOLOGICAL REVIEW

A REVISION OF IMAGELESS THOUGHT[1]

BY R. S. WOODWORTH
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Several years ago I was led by some experiments on voluntary movement to conclude that an act might be thought of without any representative or symbolic image, and further study led me to extend this conclusion to other thoughts. My attention was soon called, in a review of this work by Angell, to previous discussions of the same question, connected with Stout's assertion that there was nothing psychologically absurd in the conception of imageless thought. Looking into the contemporary experimental literature, I then made the acquaintance of Binet and of Watt, Bühler and others of the Külpe school, and my own work soon fell into insignificance beside these extensive and many-sided contributions. Even the merit of independent confirmation was not specially important in this case, since such confirmation was forthcoming even from those who, like Wundt, were not at all in sympathy with the conclusions of the imageless thought party. It appeared that imageless thought, the mere gross fact of observation, had come to stay, and that the only question was what to do with it. Some psychologists have assigned great importance to this fact as a demonstration of non-sensory content, while others have avoided so revolutionary a conclusion by explaining the fact away through one interpretation or another; others again have accepted the fact but minimized its importance, treating it as a mere limiting case; and some, while

[1] Address of the President before the American Psychological Association, at Philadelphia, December, 1914.

accepting the *gross* fact, have doubted that it would stand the test of more refined introspection. Meanwhile, my own views have been maturing as the result of continued thought and experiment, and the time is perhaps favorable for resuming the offensive, and endeavoring to uncover the weaknesses of the negative interpretations, and for offering a conception of the matter which may possibly appear superior to those hitherto presented, or at least worthy of some consideration.

Of the interpretations of imageless thought which explain the fact away without allowing it to modify existing systems of psychology, the most important is that of Wundt. It will be recalled that the method employed by the Külpe school in studying the thought processes was drastically criticized by Wundt, who objected to their experiments as being experiments in appearance only, and held that real thinking could not be done to order in the laboratory. He himself preferred to rely on incidental introspections during spontaneous thought, and in fact reports such observations of his own.[1] "In such self-observations," he writes, "it became perfectly clear to me that the thought was not formed during the process of its verbal expression, but was present as a whole in consciousness before the first word was reached. At first none of the verbal or other images, which subsequently appeared in running through the thought and giving it expression, was present in the focus of consciousness, but these parts of the thought appeared successively as the thought was allowed to develop." With only this fact in mind, he admits, one might easily be led to regard the thought as a unit with a distinctive elementary character. But quite a different conclusion is reached when other facts are also taken into account, that of the narrowness of the field of attention, that of the existence of dim content in the background of consciousness, and that of the "total feeling," itself a unit, though generated by a complex of images. A thought, in Wundt's view, is essentially a complex of images, but these parts of the thought are too numerous to be present together in the field of attention. They are present at first only in

[1] *Psychologische Studien*, 1907, 3, 349.

the background and are not introspectively visible; but as the thought is dwelt upon and expressed, its constituent images come successively into view. What then was the apparently unitary thought with which the process started? This, explains Wundt, was a "total feeling," generated by the complex of images in the background, and itself occupying for an instant the center of the stage.

It is obvious that such a position is almost inexpugnably entrenched. The extremely hypothetical nature of the ground renders a direct attack hopeless. So much as this may be ventured, that, if the words expressing a thought are really its constituent parts, it is curious that the same thought can be thought in different words, and even in different languages, and still more curious that the words to fit the thought are not always at hand. Apparently, the same complex may be composed of different elements, and may exist with some of its elements lacking. Further, it is curious to reflect that these verbal images in the background must somehow be present simultaneously and yet in proper sequence, since otherwise they might compose quite a different thought or no thought at all.

But the principal doubt to be raised concerns the "total feeling." This unitary feeling, present without observable images, and "adequate to the thought," would almost meet the demands of the opposing party, except for Wundt's insistence on its being a feeling, to the neglect of its noetic character. Certainly it is not a feeling, in any strict sense, that straightway finds expression in a statement of fact. Wundt's analysis leaves out of account the core of the whole experience, namely, the fact or supposition which was subsequently expressed in a sentence, but which was definitely and clearly present in mind in advance of the words.

Several writers have called attention to the presence of vague or apparently irrelevant imagery in moments that would otherwise appear imageless. The presence of kinesthetic sensations, habitually unattended to, has also been shown in many cases, and thus we have become wary of asserting that a given moment is really devoid of sensory

content. Of course, no one has ever supposed that bodily sensation could be absent from the background of any conscious state, but it has been thought possible to distinguish between irrelevant content and content related to the topic of thought. We must, however, recognize the probability that apparently irrelevant sensations and images sometimes enter into the web of thinking. Especially has the attempt been made with some success to extend the James-Lange theory of emotions to cover the so-called "conscious attitudes"; and some would even extend it to cover the imageless awareness of definite facts, contending that every thought has its own peculiar motor expression, and that the sensations generated by the movement furnish the conscious content of the thought; but no one, as far as I know, has found empirical support for this extreme view.

It is worth remarking that the presence of images and sensations in many or most moments of thinking does not disconcert the supporter of imageless thought. He is perfectly willing to admit that such content is often or even usually present; and the only real importance of a few well-attested instances of thought without such content is that they furnish him his most direct evidence of the existence of other content. His main contention is that other content exists, and that it is the most essential and characteristic of all.

But some psychologists, while admitting the occasional occurrence of imageless thought, deny its evidential importance. It is merely the limiting case, they say, in a continuous gradation from thought in clear images, down through thought in medium and dim images, to thought in images at or near the zero mark. The most attractive form of this interpretation is that which sees in the graded series the progressive automatization of a thought through practise. When the thought is novel, it comes with abundant sensory content, but as it grows familiar and habitual it becomes less sensuous, that is to say, less conscious, until, just as it is about to become automatic and unconscious, it still shows a feeble spark of conscious life; and this feeble spark is pounced upon by the imageless thoughter and rashly heralded forth

as proof of some unrecognized species of conscious experience. In reality, imageless thought is imageless because it is all but unconscious. This genetic interpretation has been presented with most force by Titchener[1] and by Book.[2]

The undoubted attractiveness of this conception comes from its following so neatly from the law of practise, and its deficiencies arise from its taking account of only one side of the practise effect. There is much in practise besides the tendency toward automatism. Seldom does the course of training consist of repeating time after time the same performance, only with increasing smoothness and speed. Usually the process begins with varied and tentative reactions, and advances by selection and elimination. Moreover new forms of reaction, made possible by the progress in facility, make their appearance in the course of training. Thus the perfected act omits elements present at the start and contains elements not present at the start, and may be an entirely different means of reaching the same result. If therefore the first thinking on a given topic is fraught with imagery, while the practised thought on the same topic is bare of images, it does not in the least follow that the imageless thought is a condensation of the imaginal. It may be a more economical substitute. The imagery present at the start may have been due to a diffusion of excess energy such as is common in unpractised acts, or it may have furnished a round-about way of dealing with the problem and have given place with practise to the more direct attack represented by the imageless thought.

Practise experiments give little ground for believing that a series of part acts, by simply becoming very easy and swift, blend together into a total act in which the parts are lost to sight. Rather has it been found true that the more inclusive acts, such as dealing with words and phrases as units, in typewriting and telegraphy, arise suddenly as new forms of action, in the progress of training, and themselves make possible a great increase in the speed of the partial or lower-

[1] "Experimental Psychology of the Thought Processes," 1909, pp. 173, 183, 187.
[2] PSYCHOLOGICAL REVIEW, 1910, 17, 381.

order acts. The partial acts do not blend to produce the inclusive act, but the latter is hit upon and causes the former to blend. Attention deserts the parts, which thus become automatic; but attention still remains keenly alive, being directed to the more inclusive acts. These higher acts are real units, and not mere blends; they are clearly conscious and yet not in imaginal form; indeed, they seem the very type of an imageless thought.

Observations of new ideas, at their first appearance in an individual, would be of interest in relation to the interpretation of imageless thought as exclusively old and well-drilled thought. In the hope of gathering such observations, I have sought to catch myself at moments when some new idea germinated in my mind. Unfortunately, opportunities have not presented themselves with the frequency that could be desired; but, in the few instances that I have collected the experience could be described as the dawning of some new meaning in things, sometimes with scrappy verbal and visual images, sometimes with none that were observable. When they occurred, the images were promptly forgotten, though the thought was firmly impressed on memory. So far from accepting the view that imageless thought is automatized thought, I should be inclined to believe that a new thought is characteristically imageless, and that it attaches itself secondarily to a word or other convenient symbol, and is more apt to occur with an image when it is somewhat familiar than when it is new.

Still another interpretation of imageless thought, or of the observations that purport to reveal it, presents a serious obstacle to our progress. Frequently such statements as these are contained in the subject's retrospective report: "I thought of such and such an object," or, "I thought that such and such was the case," this being the extent of the subject's description of his experience, except for the purely negative statement that no images were present. The objection has been raised by Dürr,[1] von Aster,[2] and Titchener,[3] that in

[1] *Zeitschrift f. Psychol.*, 1908, **49**, 313–340.

[2] *Ibid.*, 56–107.

[3] *Op. cit.*, p. 147.

such reports the subject is not playing the game. He has fallen from psychological description into the commonsense habit of telling what he has been thinking about. He has committed the Kundgabe or expression error: instead of describing his thoughts, he is expressing them. He has committed the stimulus or object error, and, instead of describing consciousness, is mentioning the objects with which consciousness was concerned. Confronted with this objection, the subject is apt to reply that he has done his best, that what was present in his mind was precisely the fact or object mentioned, and that if he is forbidden to refer to the object, all he can do is to hold his peace. Though this reply fails to satisfy the critic, there is something to say in the subject's behalf. Suppose, for the sake of argument, that the specific thought content exists: how would you propose to describe it? You offer the subject his choice of sensory terms, but these he rejects as not fitting the case. If then you exclude reference to objects, you have nothing further to offer him beyond a few vague and negative terms, such as "imageless," "peculiar, unanalyzable state," etc. In fine, the objection has force only on the assumption that the state should be described in sensory terms, and that non-sensory content is non-existent. It prejudges the case.

It is curious that the presence of the stimulus error in reports of images is not treated with a similar seriousness. Seldom in the literature will you find an image really described. Instead of an analysis of the visual picture as composed of colors and shadings in a certain spatial arrangement, instead of an analysis of the auditory image as consisting of a sequence of elementary sounds, you read of "a visual image of a Massachusetts town," or of "an auditory image of the experimenter saying 'subordinate concept.'" If it is committing the stimulus error to report a "thought of" such and such an object, it is equally committing it to report an "image of" the object. A strictly descriptive regimen would require the subject, one would think, to exclude all reference to the object in the one case as in the other.

Yet consider the situation of an observer who is forbidden

to refer to the object in describing his images. He would have to confine his report to such statements as "a bright, somewhat variegated spot against a dark ground," omitting to state that this was an image of his friend's face. Yet, if the image, whether faint or vivid, schematic or detailed, was for him, at the moment, an image of his friend's face, can he properly describe the consciousness of that moment without reference to his friend? No question of the logic of meaning is here involved, but a mere question of fact: Was or was not a reference to the object present in the momentary consciousness; and, if so, can the state be described without reference to the object?

The same question arises when we have a presented object instead of an image. I hear a noise from the street and say, "There is a horse galloping past." This is a commonsense reaction which makes no pretense of describing consciousness. But suppose I do attempt to describe consciousness. It is then, perhaps, in order for me to tell exactly what auditory sensations I had. If I do this as well as possible, and find nothing further, such as an image, to report—have I then, with my inventory of auditory sensations, fully accomplished my task of describing consciousness? It would seem not, if I actually was conscious of a galloping horse, while my report makes no mention of this object. It is all very well to warn me of the stimulus error if I show a tendency to go beyond my momentary experience and tell something about the horse which may be objectively a fact but was not present in my mind at the moment; but if I stick closely to the momentary experience, reference to the object is quite in order and in fact indispensable; for, as a matter of fact, reference to the object was probably the most prominent part of the experience. This is equally true in the case of an image, and I must conclude that an observer is perfectly justified in reporting an "image of his friend's face," and that he could not omit this reference to the object without badly mutilating the experience. If so, the observer who reports the "thought of such and such an object" is equally within his rights. He may have omitted something which a complete description should include, but

he has, in all probability, reported the most prominent datum of his momentary consciousness.

One further important objection to the doctrine of imageless thought is contained in the teaching of such men as James, Ebbinghaus and Dewey. In speaking of non-sensory content, we have neglected to define sensation, or, worse yet, we have, according to these authors, fallen into the error of excluding relations, forms, patterns, meanings from our concept of sensation, and then being badly put to it to explain how they get into perception and thought. It is impossible, we are told, to draw a line in sense perception between what is sensation and what is perception; and there is therefore no excuse for speaking of non-sensory content in sense perception, nor for speaking of such content as present in thinking, unless we are ready to make the improbable assertion that positive content is vouchsafed us when withdrawn from the world of sense that can never be experienced in the presence of physical objects.

Instead of attempting to meet this objection directly, I propose to go on with a positive interpretation of imageless thought, in the hope that it may avoid the difficulty, and ultimately find a legitimate ground for the distinction between sensory and non-sensory.

To reach a positive interpretation that shall have any real significance, it is essential to turn away from the isolated fact thus far considered, and seek other facts which may be brought into relation to it. A hint as to the most profitable direction in which to seek for related facts is afforded by the following consideration. Thought deals largely with data derived from past experience. New ideas may certainly be generated in the process of thinking, but in very large measure the content of thought is provided by memory; and it is usually this memory content which appears in the imageless form. It may then be profitable to bring our rather extensive knowledge of memory into relation with the phenomenon of imageless thought; and it is in that direction that I propose to search.

On examining the way in which recalled facts present

themselves, we are at once struck by something that broadens the outlook considerably. It is not only in thinking, properly so called, that facts come to mind without images, but in the most commonplace acts of memory. I recall, without visual, verbal or other observable images, what I have in my pockets, where I left my umbrella, whether my neighbor is at home today. This imageless recall is with some individuals quite the rule. The facts are clearly enough present in mind, but if there be any image it is so excessively dim as to elude detection. Such imageless recall is indicated though perhaps not fully demonstrated by some of Galton's results; and Miss Martin has recently[1] given a clear demonstration of the existence of memory content that is "unanschaulich."

In imageless thought, then, the imagelessness has nothing particular to do with the thinking process; and we are permitted to drop, with some relief, the elevated tone that has sometimes seemed appropriate to the topic. Thought is imageless because its data are recalled in an imageless form, and not because it does not thrive in a sensory atmosphere. Much effective thinking occurs in the physical presence of its object. The use of the word "thoughts" to denote non-sensory content is unfortunate, for the words "thought" and "thinking" customarily denote a certain mental function or group of functions, and cannot easily be restricted to any particular sort of content. The best word would be one that suggested recall rather than thinking; but I am not at present prepared to suggest a suitable nomenclature.[2]

[1] *Zeitschrift f. Psychol.*, 1912, 65, 417–490.

[2] Unless the following suggestion can be seriously entertained. It has long appeared to me that we psychologists were on the wrong track in our selection of technical terms. Our custom is to choose some term of common usage that may convey to the uninitiated a suggestion of the technical meaning newly attached to it. The trouble is that the untechnical usage continues alongside of the technical and tends to cause confusion; until finally psychologists are driven to exclude the untechnical use from their discourse, and thus lose a very convenient tool of expression. It is nothing less than a scandal, for example, that the word "feeling" should have been so refined in usage that the psychologist can no longer speak of a "feeling of hesitation," and scarcely of a "feeling of familiarity," without an apology and the dread of being misunderstood by his colleagues. The older sciences, with their greater need for an extensive technical vocabulary, have gone to work in quite a different way. They either take unfamiliar Greek and Latin words and derivatives, or they set apart

What, then, is it, in general, that is recalled? An old standard answer is that we recall our past experiences. Objection has several times been raised to this answer within the last two decades; but the following line of criticism is perhaps new. In experiments on testimony, or on "incidental memory," the subject is found to be incapable of recalling much that has been before his eyes, and even within the general scope of his attention. If he could call back his original experience, it would seem that he could give the testimony required of him. A specially instructive experiment, for our present purpose, is that of Thorndike,[1] who asked his subjects to call up an image of a certain scene, as of the front of a familiar building, and then, after they had estimated the vividness of their images, asked them specific questions, as to the number of pillars in the facade and similar details. He found a marked inability to answer the specific questions, even on the part of individuals with very lifelike images; and, in fact, there was little or no correspondence between vividness of image and correctness of report on details. I have frequently repeated this experiment with the same results. I have never found an individual able to read off the number of pillars from his image. Only those could tell the number who had at some time counted them; and other subjects protested that it was not fair to expect them to find the number of pillars in the image, when they had never counted them in the original. All this seemed highly suggestive. It suggested that only that was recalled which had been noted in the original experience; and that even vivid

some proper name to serve the special purpose. Thus they have their watts and volts and ohms and amperes, terms regarding the meaning of which no one need ever be in doubt. Such terms are much better than "thoughts," or than "Bewusstseinslagen," with its doubtful translation of "conscious attitudes." I would propose, accordingly, to follow the lead of physics and chemistry; and since Bewusstseinslagen were first reported and defined in the work of Marbe and his associates, I would suggest calling them "marbs," the term to be defined for all time by reference to the original description by Marbe. Similarly, since the "thoughts" were gradually brought to light by the school of which Külpe was the guiding spirit, I would suggest calling them "kulps," defining this term similarly by reference to the original works. These terms are certainly beautifully compact and euphonious, and those who can bring themselves to use them will find them very convenient.

[1] *J. of Philos.*, 1907, 4, 324.

images, described as being fully equal to the actual experience, were in fact something quite different.

I was thus prompted to undertake an examination of images and other content of recall, in order to see how far they could be described as revivals of past experiences, and how far they consisted of facts noted in the past. I set myself to recall events from my past life, and in other cases to recall persons, buildings, towns, and such specific facts as the exact colors of postage stamps, the quality of a friend's voice, the shapes, tastes, odors, etc., of a great variety of objects. What I got was sometimes to be called an image and sometimes not; but in all cases, with a few doubtful exceptions, it consisted of facts previously noted. When I say "facts," I do not mean verbal statements of facts, but a direct consciousness of some thing, quality, relation, action—of something which I had observed in the original experience. I did not get back experiences as concrete totals, but only facts which I had discriminated out of those totals. In the original experiences, those facts had had a concrete setting or background; but this setting was not recalled. The facts were recalled in isolation.

Often, indeed, a rudimentary setting was present, consisting of either a personal reference, or a spatial reference, or both. By "personal reference" is meant that the fact was recalled as my own experience, or that the relation of the fact to me, or my attitude to it, was recalled along with the fact. By "spatial reference" is meant that an object was recalled as being to the right or left, or in a certain town, or in a certain direction from my position at the time of recall. Spatial reference was more frequently present than personal. Neither was universally present; and, aside from them, no setting was recalled. It frequently happened that several facts derived from the same experience, or from different experiences, were recalled almost or quite simultaneously, so that the recall was richer than would be suggested by the expression, "isolated fact." Nevertheless all of these facts had been previously noted, and they did not bring their concrete setting back with them.

As an example of my results, I will cite the recall of a colleague speaking in faculty meeting. What I got was a certain quality of voice and precise manner of enunciating, rather different from the conversational tone of this individual. There were no words nor particular vowel or consonantal sounds present in recall, but simply the quality of the voice and enunciation. I got also the fact that the speaker was speaking as chairman of a committee, and something of the rather critical attitude of the faculty towards him, these facts being recalled in the "imageless" way. Besides, I got a spatial reference, in that the speaker was located in a certain position with respect to my position in the meeting; and a vague personal reference amounting to an attitude of support or well-wishing. Beyond this, nothing. No visual background of faces or furniture, no auditory background of words spoken, no somesthetic background of myself sitting.

Among the facts thus recalled in relative isolation and without concrete setting were the following:

Of persons: shape of head or of nose, breadth of face, color of eye, curliness of hair, blotchiness of complexion, facial expression, tone of voice, trick of gesture, "smoothness" of manner, social position, ability, industry, relation to myself, as being friendly or unfriendly, a superior or dependent, agreeable, a bore, etc., or as having been seen recently or long ago.

Of buildings: location, size, color, material, architectural style.

Of towns: location, general topography, old or new style, abundance of shade, holiday atmosphere, quietness, association with certain events.

These facts run the gamut from simple to complex, and from sensory to abstrusely relational. They are so varied as to indicate that any observed fact can be recalled in isolation. Among the striking instances of isolation were recall of the color of an object without its shape, of its shape without its color, of its gloss or shading without either color or shape.

The following interpretation seems scarcely more than a restatement of these results. An actual situation presents an

almost unlimited variety of facts or features, of which an observer notes a few, the rest remaining undiscriminated in the background and giving the concrete setting of the features noted. Later, he may "remember" the situation, but this is not to reinstate it in its original multiplicity and continuity. He recalls the features which he observed, or some of them, but not the great mass of material which remained in the background. Lacking this setting or background, he is not in a position to make any fresh observations in recall, and thus arises the weakness of incidental memory.

If generalized to cover all cases in all individuals, this statement does indeed go beyond the evidence at hand. But if the possibility of an occasional recall of the concrete setting is left open, and the assertion simply made that an observed fact is often recalled without its original setting, this conclusion, though modest, is sufficient to furnish a positive interpretation of imageless recall.

Were it true that a recalled fact always brought with it its original setting, then, indeed, all recall would involve sensory imagery. But if a fact is recalled in isolation, it depends on the nature of the fact whether the recall would be called imaginal or imageless. If the fact lay as it were on the sensory surface of things, such as color or tone, its recall would usually be spoken of as an image. If the fact lay below the sensory surface, as the fact that a speaker was exaggerating, or speaking as chairman of a committee, an isolated recall of this fact would be unhesitatingly pronounced imageless, unless, to be sure, it were accompanied by a verbal or symbolic image derived perhaps from another source than the original setting of the fact. The definitely imaginal and the definitely imageless are the extremes of a series, between which lie many intermediate facts difficult to place in either class. The expression of a face, the composition of a painting, the style of a building or piece of music, recalled in an isolated way, are difficult to classify.

If you set yourself to discover what are the objects of your attention in a sensory experience, you will usually find that the actual sensations are less prominent than the things signified

by them. You are more conscious of the horse galloping past than of the actual noises that you hear. When, therefore, you later recall hearing a horse gallop past, it is not surprising that the thing signified should be recalled more distinctly than the noises; and you are left in doubt whether to class the recall as an image or not. This is a type of numerous cases. An observed feature of a situation often lies partly "on the sensory surface" and partly below, and the observer does not take separate note of the sign and of the thing signified, but perceives them together as a single fact. His recall of the fact may then partake both of the sign and of the thing signified, though the sensory flavor is usually weakened in recall. The distinction between imaginal and imageless, between sensory and non-sensory, is not perfectly sharp, and appears, from our present point of view, to be of minor significance, the main principle being the isolated recall of observed facts.

I ought really to rest content with the conservative statements that precede, and leave imageless recall as an incident to the occasional, or frequent, recall in isolation of previously noted facts. But in the interests of a more clean-cut theory, I am tempted to more radical and general statements. I propose to strike out boldly and formulate a theory, hoping that, whether acceptable or not, it may prove a stimulus to thought and perhaps to experiment.

The first step towards this theory is to generalize the conclusion derived from observations already cited, and to offer the hypothesis that all recall is of facts previously noted, freed from the concrete setting in which they occurred when noted. This generalization I hold to be correct for my own case, and, though the testimony of many individuals regarding their imagery is on its face in flat contradiction with mine, the objective test of incidental memory seems to show that there is something radically wrong with their testimony. My generalization has the advantage of squaring with the facts of recall as objectively tested, and the only difficulty is to explain away the introspective reports of images "fully equivalent to actual experience," and of "living over the past as if it were present."

Without pretending to do full justice to this testimony, I must for the present content myself with a few remarks. Undoubtedly a person may become deeply absorbed in a remembered experience, because of its great interest for him. Now his present interest is probably the same as that which dominated him in the original experience and led him to observe and react to certain features. If, his interest reviving, he gets back these features and reactions, he has the essentials of the original experience from his own point of view, and satisfactorily lives it over again, even without the concrete background, the absence of which, in his absorption, he would not notice, any more than he noted its presence in the original experience.

As to the vivid image, said to be "in all respects equivalent to the actual scene," we undoubtedly have, in such a case, a revival of personal attitude and emotional value, which alone are enough to create a strong atmosphere of reality. We must also recognize that what an artist might call the general effect of a scene is as much a fact to be observed as any other. The features which can be analyzed out of a situation are not exclusively details, but include broad effects and syntheses and anything that can be the object of attention. If now you recall the emotional value and general effect of a scene, along with some of the colors and other previously noted details, you perhaps have enough to make you testify, rashly, that your image is in all respects equivalent to the actual scene. A test of incidental memory would soon convince you that the "equivalence" is an illusion.

It is also true that a person may observe a scene in such detail as to recall a great number of its features; and he might express the wealth of his recollection by asserting that he revived the entire experience; but, so long as what he recalls is what he previously observed, he offers no exception to the rule that has been formulated.

We have not yet by any means exhausted the relevant information to be derived from studies of memory. Evidently we should be much helped in any study of recall by having at hand a report of the process by which what is now

recalled was originally learned. We should be helped in our present inquiry by knowing whether "impressing a thing on the memory" consists in simply standing before the thing and letting it "soak in," or whether it consists in reacting to the thing by observing its characteristic features. It may be said at once that studies of memorizing give little sign of a purely receptive attitude on the part of the learner, and much evidence of a reactive and analytical attitude. Meumann emphasized the importance of the "will to learn." A subject might attentively examine a list of nonsense syllables, and yet make little progress in memorizing it unless his will to learn were excited. Now the "will" can scarcely be conceived as acting without means or tools; and its tools consist of various specific reactions to the matter set for memorizing, the reactions varying with the material and with the test of memory that is to be met. Some of these reactions may properly be called motor; here would be classed the rhythm, accents, pauses and vocal inflections that are read into the list by the learner. But in large measure the reactions are of the perceptual sort, and consist in observing positions, relations, patterns, meanings, in the matter to be learned. The recent studies of Müller throw all these factors into clear relief. Memorizing is very largely a process of observation, of noting those features of the material that will serve to hold it together in the desired way. Some of these features, such as patterns and relations and the nearer-lying meanings, are, as it were, found in the material itself; while other features, the more far-fetched meanings and associative aids, are imported from without; but this distinction is only one of degree.

The reactions made in learning, it should once more be said, are specific, and adapted not only to the material learned but also to the kind of memory test that is anticipated. If the subject expects to recite a list of words or syllables throughout, he observes positions, sequences, patterns and relations that will serve to bind the whole list together. If he expects simply to respond to each of the odd-numbered words in the list by giving the following word, as in the method of paired associ-

ates, he takes each pair as a unit, and observes characteristics of the pair that bind it together, but neglects the sequence of pairs. If he expects to be called upon to recognize the individual words of the list, he fixes his attention on them singly, observing in each, as far as possible, some character that may serve to impress it. There is no one uniform process of learning, and the will to learn cannot be conceived as a general force or agency. What we find in memorizing is a host of specific reactions, largely of the perceptual sort.

I may be permitted to cite the results of a little experiment designed to test this matter. I read a list of twenty pairs of unrelated words to a group of 16 adult subjects, instructing them beforehand to learn the pairs so as to be able to respond with the second of each pair when the first should be given as stimulus. But, after reading the list three times, I told them that they should, if possible, give also the first word of the following pair on getting the second word of the preceding pair as stimulus. I then read the first word of the list, waited 5 seconds for the subject to recall and write the second word; then read this second word, and waited the same time for them to recall and write the the third word, namely, the first word of the second pair; and so on through the list. The results were most definite: the second members of the pairs were correctly recalled in 74% of all the cases, but the first members were recalled in only 7% of the cases. The subjects reported that this great difference was apparently due to the fact that they had examined each pair with the object of finding some character or meaning in it; whereas they had neglected the sequence of pairs as being of no moment.

This result is instructive in several ways. It indicates, first, that the will to learn operates not by favoring a general receptive or memorizing attitude, but by leading to specific reactions of the observational type. It serves, next, to fortify the results of other experiments on "incidental memory." Here the objection cannot be raised that the incidental matter that is not recalled was never attended to; for the first words of the pairs were attended to as well as the second. The experiment also shows the unsatisfactory character of Ward's

conception of the process of learning. He has said that associations are formed by the movement of attention from one to the other of the terms associated. But here attention moved from the first to the second member of a pair, and thence to the first member of the next pair; yet the first movement seems to have established a strong association, and the second, comparatively speaking, none. Evidently something much more specific than a mere movement of attention has been in play. The members of a pair are associated by the sequence, connection or meaning that is found in the pair. Finally, this experiment serves to strengthen doubts that have often been raised, especially by the work on incidental memory, regarding the adequacy of contiguity in experience as an associating force. Here the contiguity between the members of a pair was scarcely greater, in matter of time, than that between successive pairs; yet the association within pairs was strong, and that between successive pairs almost negligible. Since the associations within pairs gave 10 times as good a score as those between pairs, we may perhaps say that mere contiguity does not contribute more than one tenth of the whole associating force, the remaining nine tenths being contributed by the noting of suitable features in the material. Even the small fraction thus left to contiguity does not necessarily belong to it; for it is not improbable that the sequence and relation of successive pairs were sometimes observed. In fact, of the few correct recalls of first members, practically all occurred at the beginning or end of the list of twenty pairs; and it is quite likely that, in these favored positions, attention was occasionally directed to such incidental matters as the sequence of pairs or their positions in the list. Except at the ends of the list, the score for first members was only 1/85 as good as that for second members of the pairs; and this fraction, rather than 1/10, probably represents the proportion of the total associative force that should be assigned to mere contiguity; though even this is a doubtful concession.

It may be considered superfluous bravery in me to challenge the doctrine of association by contiguity, in addition to all the other enemies already on my hands; but, in reality,

I have this doctrine on my hands at any rate. For if contiguity in a momentary experience is a strong and sufficient associative force, then any item that is later recalled will in turn recall its contiguous items and redintegrate the whole experience or a large part of it, and my hypothesis that what is recalled is observed facts without their setting would become untenable.

Now association by contiguity has played a worthy and important part in the development of psychology, and its attempt to absorb into itself all other laws of association has, in my opinion, been a success. Things become associated only when they are contiguous in experience. That is to say that contiguity is a necessary condition of association. But is it a sufficient condition? There is little in the experimental work on memory to indicate that it is sufficient, and much to indicate that it is not usually depended on to accomplish results. The things to be connected must be together, in order to arouse the reaction connecting them; but, unless they arouse some such reaction, they do not become connected, except it be very weakly. The reaction may be described in a general way as a reaction to the two things together; it is perhaps sometimes a purely motor reaction, but most often, I believe, is rather to be called a perceptual reaction, consisting in the observation of some relation between the two things, or some character of the whole composed of the two taken together. In any case, the reaction is specific; and it is this specific reaction, rather than any general factor like contiguity, or the movement of attention, or the will to learn, that does the work of association. To judge from the memory experiments, then, what is recalled is what has been noted—not past experiences in their totality, but definite reactions which occurred in those experiences.

This conclusion is perhaps even more clearly indicated by experiments in the learning of nonsense drawings than in the more usual work with linguistic materials. An instructive experiment is that of Judd and Cowling,[1] who exposed a rather simple drawing for successive periods of 10 seconds,

[1] "Studies in Perceptual Development," Psychol. Rev. Monograph 34, 1907, 349–369.

requiring the subject to reproduce it as well as possible after each exposure. The results, both objective and introspective, showed that the subject usually got first the general character and shape of the figure, and, continuing his analysis, noted one fact after another, until a sufficient number of facts was known to make a satisfactory reproduction possible. There was no evidence of an inner reproduction of the entire sensory experience, from which the subject might read off such information as he required. In a somewhat similar experiment, T. V. Moore[1] called for the learning of a series of simple drawings. He supposed at the outset that a group of figures would be memorized by visual imagery, but experience taught him that there was another factor that was a powerful aid to memory. This was "a more or less complete analysis of the figures, an analysis which it is utterly unneccessary for the subject to put into words." It consisted in noting the parts and composition of the figures and their resemblances to familiar objects. He then undertook to compare the efficiency of memorizing by visualization with analysis excluded, and by analysis without visualization; and found a uniform superiority of the analytic method over the visualizing. But he also found that it was impossible to exclude analysis altogether. "Associations crop up spontaneously," he writes, "and one simply cannot exclude all analysis of the figure. . . . It is much easier to memorize by analysis to the exclusion of imagery than *vice versa*." He believed, however, that learning by visualization, i. e., by forming an image which should be a "more or less perfect replica" of the visual sensation, was a real process. Under the circumstances, it was evidently impossible for him to prove this; for if analysis occurred spontaneously—and one has only to look at a drawing to realize how inevitable it is to note either details or broader characteristics—and if also analysis was a more powerful memorizing agency than visualization, it remains possible that all the learning was accomplished by analysis. The reality of the strictly visualizing or photographic process of learning is, I believe, still open to doubt. It is certainly

[1] "The Process of Abstraction," *Univ. of California Publications in Psychology*, 1910, 1, 139–153.

impossible to avoid perceptual reactions, and to assume the purely receptive attitude of a photographic plate.

Miss Fernald's data on the memorization of pictures[1] show that even good visualizers depend largely, at least, on specific observations of the features which were later remembered; and her results on the recitation of letter-squares in changed orders[2] showed that even the best visualizers among her subjects were unable to do what it had been supposed was the prerogative of a visualizer to do, namely, "see the whole set of letters at once and simply read them off" in the changed order. She does not doubt the existence of persons able to accomplish this feat, but believes that they must be rare. This matter of visualization evidently requires further study, but the possibility is still open that even the best visualizer does not carry away a photograph of the scene, or replica of his visual sensation, but an image which amounts to a synthesis of specific observations, including observations of broad effects and observations of parts and their relations.

But it is time that I brought my theory out of hiding and placed it squarely before you. I call it, for lack of a better name, the mental reaction theory, or perhaps the perceptual reaction theory. Its basic idea is that a percept is an inner reaction to sensation. I call it a mental reaction to distinguish it from the motor reaction which several psychologists have put forward as being important in attention, perception, association and the like; for it appears to me that these suggestions, while on the right track in insisting that *reaction* is dynamically important, have mistaken the locus of the reaction, and so are unable to account for the conscious content that appears in these mental activities. This mental reaction is not, however, of the nature of an associated sensation, appearing as an image, as if the visual sensation of an orange, to give the percept orange, must reproduce the sensations of handling or tasting the orange. Nor, on the other hand, is the perceptual reaction an emphasis or pattern or meaning residing in the given sensations. It is something new, not present in the sensations, but, theoretically, as

[1] Psychol. Rev. Monograph 58, 1912, 81ff.

[2] *Ibid.*, p. 71.

distinct from them as the motor reaction is. It adds new content which cannot be analyzed into elementary sensations; so that the sensory elements, which are often held to supply, along with the feelings, all the substance of consciousness, in reality furnish but a fraction of it, and probably a small fraction. Each perceptual reaction is specific, and contributes specific content. In recall, it is these perceptual reactions that are revived, and not sensation; and therefore the content of recall is never, in the strictest sense, sensory. Nevertheless, as was said before, some percepts lie, as it were, nearer to sensation than others, so that the distinction between an image and an imageless recall, while not perfectly sharp, is still legitimate.

It is possible that this theory may appear not so radical after all, and not worth the expenditure of so much breath; for all will perhaps admit that a percept is, in some sense, a reaction. It is therefore my duty to show that the theory is worse than it seems, and this I shall attempt to do in the case of patterns or Gestaltqualitäten. It has long been known that the same pattern (for example, a melody) can sometimes be found in different sensory complexes, and it is also true that different patterns can be found in the same sensory complex, as in the case of the dot figure. A rather difficult problem is thus raised, for one would think that the compound would be determined by the elements. But the real crux of the difficulty is to get some conception of a pattern or of a compound, to show what is meant by the togetherness or grouping of the elements. There are three theories that attempt to solve this puzzle, that of synthesis, that of systasis or mere togetherness, and that of synergy, which is none other than the mental reaction theory. The synthesis theory brings in the subject or ego to put the elements together; the systasis theory rejects this deus ex machina, and says that the elements merely are together, or get together and so constitute the compound or pattern; the synergy theory holds that the elements act together, as stimuli, to arouse a further reaction which is the pattern. The synthetic theory occupies a weak position, since, unless the systatic theory succeeds in showing

what is meant by the elements being together, there is no advantage in saying that something puts them so.

Now it is difficult to understand what can be meant by the elements being together or getting together so as to produce the group and pattern. If the group included the whole momentary content of consciousness, we could say that being together meant simply being simultaneously present, and speak of the pattern as a character of the whole conscious moment. But the group does not include the whole of consciousness, but—as in the case of three dots among a larger number, seen for an instant as a triangle—may occupy but a small part of the conscious field. The pattern is not the pattern of consciousness, but a pattern within consciousness. Nor will it help matters much to substitute for consciousness the field of attention; for the extent of a group may be either greater or smaller than that of this field; and, besides, a familiar pattern, such as a melody or arrangement of lines or dots, may come to consciousness quite outside the field of attention. Apperception, then, in the Wundtian sense, does not explain groups and patterns nor give them any intelligible meaning. But if we lay aside apperception and try to describe groups and patterns in terms of their constituent elements, we are in no better case. What is it that changes when the pattern changes, the elements remaining constant in quality, intensity and spatial position? This question is as serious for the synthetic theory as for the systatic. The synergy theory cuts the Gordian knot by admitting at once that there is no change in the elements. In fact, there is no real grouping or pattern of the elements; they neither get together nor are put together by some higher agency; but some of them simply act together, as a complex of stimuli, to arouse a perceptual reaction which constitutes the grouping and pattern. The pattern is numerically distinct from the elements, as a motor reaction is distinct from the complex of stimuli that arouses it. What pattern shall be aroused at any moment depends on the readiness of different perceptual reactions to be aroused, and thus on such factors as frequency and recency of past exercise, fatigue and present interest and control. In short,

the synergy theory proposes to extend to patterns, and to all percepts, the same explanation that is accepted for such admittedly mental reactions as the sequence of one idea after another. No one doubts that one idea may represent a stimulus for the arousal of another idea, nor denies that the aroused idea is numerically distinct from the stimulus idea and adds new content to it. It is the same with sensation and perception, except that the reaction is usually very prompt and the perceptual content intimately fused with the sensational. The fusion is so complete that the pattern seems to lie right in or among the dots, as the galloping horse of an earlier illustration seemed to be actually heard in the series of noises.

But now, finally, I suspect that the party, which allowed me to proceed some time ago without coming to terms with their demand for a definition of sensation, will no longer be restrained. They will insist on taking the floor and addressing you as follows: "The speaker is certainly right in calling a percept a reaction; that is too obvious a fact to need discussion. But we ask, A reaction to what? And our answer is, To the physical stimulus. This 'sensation' that the speaker has interpolated between the physical stimulus and the percept is pure gratuitous assumption. There is no warrant for it in introspection, for he himself admits that the sensation and the percept content are intimately fused. We regret that he has fallen into this obsolescent way of speaking, and would suggest that, in reviewing his remarks, you use the blue pencil of the censor wherever the word 'sensation' occurs."

This objection is almost too serious to be dealt with in brief. I should freely admit that sensation and percept cannot be distinguished by direct introspection. Yet there are introspective facts that make the distinction appear legitimate. When we hear the galloping horse, we are not only aware of the horse, but we are able to state that we hear him. It is not quite correct to say that we get only the meaning, for we know also the sense by which we get the meaning. So, again, when we have changing percepts of the

same stimulus, as in the case of the dot figure, the change of pattern does not amount to a complete change of the figure, but there is a constant substratum underlying the changes; and it seems appropriate to speak of this as sensation. In recall, even the best images lack something when compared with actual sensory experience. They lack body and incisiveness; and it appears probable that this lack is nothing more nor less than a lack of sensation, or, in other words, that the real sensory process is not resuscitated in the image.

But the concept of sensation might never have arisen in a purely introspective psychology. At bottom it is a physiological or psychophysical concept. Sensation is that conscious content which is in closest relation to the physical stimulus. It is the primary response to the stimulus, and may be followed by secondary responses. Neurology gives good ground for such a distinction, in tracing the sensory nerves to certain limited areas of the cortex, and finding the rest of the cortex to be only indirectly connected with the sense organs. Destruction of the cortical receiving station for any sense abolishes all conscious use of that sense, while destruction of neighboring areas, without making a person blind, for example, abolishes his power of reading, or his power of recognizing seen objects, or his power of orienting himself in visual space. Such perceptions are apparently secondary reactions, while the primary reaction, corresponding to the activity of the receiving station, is precisely that which distinguishes a person who is word-blind and object blind, from one who is totally blind. Here is a person who sees without perceiving, and here is one who does not see at all. The difference I would like to call sensation. Sensation, accordingly, would be the consciousness attending the activity of the sensory receiving stations of the brain, while percept-content would be the consciousness attending the activity of neighboring areas. Besides these secondary reactions, there are undoubtedly tertiary and further reactions, less and less directly connected with the incoming sensory impulses. They need not have a sharply limited localization in the cortex, yet they must be neurologically distinct, and it may well be

that every distinct cerebral reaction is attended by its peculiar conscious content. I know of no reason in neurology or psychology for supposing that the elements of conscious content are contributed solely by the sensory receiving centers.

According to this theory, the sensation aroused by a physical stimulus must precede the secondary or perceptual reaction; but the interval need not be supposed to exceed a hundredth of a second, and could not be introspectively detected. The fusion of the primary and secondary reactions in consciousness is a fact which I cannot attempt to explain, since fusion is one of the fundamental peculiarities of consciousness as contrasted with its cerebral correlates. But I may perhaps make the whole conception a little more tangible by reverting to the similitude of photography.

A certain photographer found himself without sensitive plates, though with his camera, in the presence of a scene which he much desired to preserve. He therefore focused on the ground glass at the back of his instrument, and, stretching transparent paper over the glass, traced some of the outlines of the optical image. He thus created patterns, which lay really in his drawing and not in the optical image, but which were blended with the image as long as the image remained. He preserved his tracing, and found it to differ from a photograph in containing only the facts to which he had definitely reacted.

In this parable, the optical image is sensation, which is gone forever when the physical stimulus ceases. The tracing is perception, which may be preserved, though subject to decay. But the fusion of the two, depending in the case of the camera on the presence of the photographer's eye, is in the case of sensation and perception more deep-seated and inexplicable. Finally, the photographer was more restricted than is the process of perception, since he could only trace outlines and shadings and perhaps colors, and could not commit to his drawing the more remote relations and meanings which can be perceived, and, being later recalled, furnish the content of "imageless thought."

A NEW MEASURE OF VISUAL DISCRIMINATION[1]

BY KNIGHT DUNLAP

The tests on which this report is based were carried out in the Nela Research Laboratory in August and September of 1914, as an adjunct to other experimental work which will be reported later. The instrument used was constructed at The Johns Hopkins University several years ago, and as a result of work with it since that time has been modified into the present form, which seems good in principle although the mechanical operation may still be improved.

The instrument which, for convenience, may be called a *duoscope*, consists essentially of a polished crystal of Iceland spar mounted in a telescopic brass tube, which has an eye-aperture at one end, and a rotatable ring at the other. The ring is rotated by a worm-screw with a knurled head, and is provided with a vernier scale, so that the angle of rotation may be read to one fifth of a degree. The ring is arranged to hold a disc fitting within it, so that various forms of objects may be viewed at a fixed distance from the eye.

The first objects tried were as near linear as possible: a diamond-scratch on a clear glass disc; a fine glass filament crossing a circular aperture in a metal disc. These were tried against various backgrounds, and were not satisfactory because of the difficulty of securing a line of sufficient uniformity and without sheen. A narrow slit was equally unsatisfactory. Finally a rectangular aperture of appreciable width was found workable when used against a bright background.

The Iceland spar crystal gives a double image of the line (or rectangle) used as an object, and the relation of these two images may be altered by rotating the ring which carries the line with it. If the line stands exactly in the refracting

[1] From the Nela Research Laboratory, National Lamp Works of the General Electric Company.

plane of the crystal, the two images are superposed over their greater length, their displacement being longitudinal only. If the line be rotated 90° from this position, the two images are displaced laterally to the maximal distance possible from the prism (1.089 mm. with the crystal used in the present work), and if this distance be greater than the width of the line (or rectangle) the two images are separated.

In this way, with a proper linear object, it would be easily possible to measure (in terms of the visual angle) the displacement of the images giving just perceptible doubleness; *i. e.*, the minimum visibile. The advantage of the device lies in the exactness of measurement and ease of manipulation, with the possibility of accommodation for relatively short or 'reading distance.' It is possible to obtain a suitable linear object, but before I had found one I discovered that observation on a rectangle of appreciable width (*i. e.*, a relatively wide slit) is much easier, and have therefore adopted such an object for the present. The fineness of measurement possible with this instrument is indicated by the fact that the reading unit in the vernier scale (one fifth of a degree) corresponds, in the middle of the scale used in the present work, to 1.6″ of visual angle, or .0029 mm. lateral displacement of the image.

The discrimination of doubleness in lines is of course a matter primarily of difference-sensibility for brightness. If (in the case of two bright lines, or two images of a single objective bright line), there is a perceptible dark stripe down the middle of the combined lines, they are seen as two; if there is no dark stripe, as one. The three factors involved in 'visual acuity' as tested by the linear method[1] are therefore: (1) The physical distribution of the light-flux on the retina, determined by the 'resolving power' of the eye; (2) The distribution of energy or activity in the physiological image of the retina, determined by the distribution, in the physical image, and by irradiation, etc.; (3) The difference-sensibility for brightness differences. Anything which changes any of these factors, as a change in the lens system, or in the irradi-

[1] In the case of determinations by means of two points instead of two lines, the situation is different, and the histological texture of the retina may be a factor.

ation, will therefore change the 'acuity,' although the difference-sensibility remains the same. Thus the practical usefulness of the acuity-determination depends on thorough (and perhaps unattainable) control of the conditions of observation. These matters are so obvious that no further discussion is needed here.

The method of testing 'acuity' by the double images of a bright rectangle is now apparent. If the two images (physical) overlap sufficiently, there is a brighter line in the middle of the combined images; if they are sufficiently separated there is a darker line in the middle. It is a relatively simple matter to determine the points at which the dark line and the bright line are *just* perceptible.

The crystal used has a maximal image-separation of 1.089 mm., and the slit was fixed at a distance of approximately 36 cm. from the eye. The slit was 3 cm. long and 0.77 mm. wide.

The most difficult part of the adjustment of the instrument is the determining of the position in which there is no lateral, but only longitudinal, displacement of the images. This determination was made by long series of observations of the bright line obtained by moving in each direction alternately, determining the middle point from these. The zero point thus obtained is sufficiently exact for practical purposes; besides, no more exact method is available.

The background against which the slit was viewed was a plane disc of plaster surfaced with magnesia, at a distance of 97 cms. from the rectangle.[1] This was used first in a darkened room and illuminated with a beam of light from a nitrogen tungsten lamp. The lamp being enclosed, the only illumination of the room was the light diffused from the disc. From the direct radiation of this light the observer was protected by a black screen, through which the instrument protruded. The observer was therefore not in complete darkness, but the illumination in his direction was low (1/27 to 2 c.p.). Ten minutes or more was allowed for adaptation, so that the subject was really in a fair state of darkness adaptation.

[1] Tests were made at other distances, but as was expected, the distance proved not to be a factor in the results.

Subsequently the instrument and plaster disc were moved into a room well lighted with daylight, so that measurements were obtained with daylight adaptation.

In the darkened room five illuminations of the plaster background were used, giving brightnesses of 3, 10, 36, 82 and 168 candles per square meter.

The observers were: laboratory helper Mr. Eric Martienssen, a high school graduate; Dr. P. W. Cobb; Dr. H. M. Johnson; and myself. The readings on me were taken by Martienssen; the readings on the other observers were taken by me. Usually twenty-five determinations were made in one sitting. Thus, in the work with five intensities, five determinations were made on each intensity at a sitting, with only one sitting a day. Each subject had preliminary practice in observing. These intensities were taken in a different order on different days. No practice effect is noticeable in the measurements of any of the subjects.

The observer started with the bright line plainly visible and rotated the slit until the dark line was just visible: then he rotated the slit in the other direction until the bright line was just visible: or *vice versa*. The observers found it easier to make the changes rather quickly. Long looking caused the difference in brightness to disappear.

It is unfortunate that the instrument was made with the crystal fixed, and the object rotating. It was so made because this form allows easier construction, and has advantages in adjustment of the object for the zero point which was desirable while the instrument was in the provisional stage. The next instrument will be made with fixed object-holder, so that the axis of the slit will not change during observations.

The rotation of the axis in these experiments was small, however, and does not vitiate the results. In the table below, where the axis is not specified, it was 90°, that is the slit was vertical when in the medial position, *i. e.*, in the position in which the images were not displaced laterally.

The readings given in the table are the displacements from the zero position in visual angle computed from the averages of the designated number of readings on the scale

of the instrument. Theoretically, the visual angle should have been computed for each instrument-reading, and then the averages of the computed values taken; practically, the computation for the average of the instrument-readings is sufficiently accurate. The mean variations are not given, because I have so far not been able to discover what the true mean variations are. It is evident that the variations cannot be referred to the averages, because these vary with the width of the slit employed, regardless of the uniformity of observations; nor to the average of the range from dark line to bright line, because there may be variations in the readings which do not affect this.

Table I

The Influence of Brightness and of Adaptation

	1. *Martienssen.* *Left Eye*			2. *Johnson.* *Right Eye*		
	A. Dark Adaptation. Av. of 25			*A.* Dark Adaptation. Av. of 25		
Brightness	Dk. Line	Br. Line	Range	Dk. Line	Br. Line	Range
3	7′ 10″	6′ 44″	26″	6′ 59″	6′ 46″	13″
10	7′ 10″	6′ 49″	21″	7′ 1″	6′ 49″	12″
36	7′ 10″	6′ 50″	20″	6′ 59″	6′ 49″	10″
82	7′ 10″	6′ 53″	17″	7′ 00″	6′ 49″	11″
168	7′ 0″	6′ 54″	13″	7′ 2″	6′ 50″	11″
	B. Daylight Adap. Av. of 20			*B.* Daylight Adap. Av. of 20		
	7′ 2″	6′ 51″	11″	6′ 59″	6′ 47″	12′
	3. *Cobb.* *Right Eye*			4. *Dunlap.* *Left Eye*		
	A. Dark Adap. Av. of 10			*A.* Dark Adap. Av. of 25		
3	6′ 50″	6′ 27″	23″	7′ 10″	6′ 39″	31″
10	6′ 53″	6′ 31″	22″	7′ 11″	6′ 48″	23″
36	6′ 49″	6′ 26″	23″	7′ 11″	6′ 49″	22″
82	6′ 51″	6′ 34″	17″	7′ 11″	6′ 50″	21″
168	6′ 52″	6′ 33″	19″	7′ 12″	6′ 50″	22″
				B. Daylight Adap. Av. of 75		
				7′ 4″	6′ 49″	15″

In the *A* parts of Table I are given the general results of the tests with different brightness of slit under darkness adaptation, and in the *B* parts, the corresponding results with daylight adaptation. Two points are clear. First, that in general the daylight adaptation gives greater acuity; and

Table II

Influence of Angle of Axis of Rectangle

1. *Dunlap. Right Eye*

A. Daylight Adap. With lens correction. Av.of 20

Axis	Dk. Line	Br. Line	Range	Dk. Line	Br. Line	Range
80	7′ 2″	6′ 46″	16″			
90	7′ 2″	6′ 45″	17″			
100	7′ 2″	6′ 42″	20″			
125	7′ 2″	6′ 42″	20″			
170	7′ 6″	6′ 50″	16″			

B. Daylight Adap. Without lens Av. of 10 — *C.* Dark Adap. Without lens Av. of 40

80	7′ 4″	6′ 43″	21″	7′ 4″	6′ 40″	24″
125	7′ 10″	6′ 32″	38″	7′ 17″	6′ 22″	55″
170	7′ 9″	6′ 55″	14″	7′ 13″	6′ 46″	27″

2. *Martienssen. Daylight Adap. Av. of 20*

A. Right Eye — *B.* Left Eye

90	7′ 9″	6′ 53″	16″	7′ 2″	6′ 51″	11″
135	7′ 00″	6′ 48″	12″			
180	Unable to	see lines.		7′ 8″	6′ 54″	14″
67.5	Unable to	see lines.		7′ 43″	7′ 22″	21″

3. *Johnson. Daylight Adap. Av. of 10*

A. Right Eye — *B.* Left Eye

90	(6′ 59″)	(6′ 47″)	(12″)	6′ 59″	6′ 45″	14″
135	6′ 55″	6′ 46″	9″	6′ 59″	6′ 47″	12″
180	7′ 00″	6′ 48″	12″	6′ 58″	6′ 45″	13″
67.5	7′ 00″	6′ 48″	12″	6′ 59″	6′ 46″	14″

second, that there is no uniform influence of brightness within the limits of the conditions obtaining.

The results with the lowest brightness differ appreciably from the results with the higher brightness, but this is due, in part at least, to the difficulty in judging with this illumination when near the line-threshold. This is a condition which must be distinguished carefully from the raising of the threshold as such, and was clearly a factor in my own case. Leaving out the dimmest light, the influence of the brightness is negligible for Cobb, Johnson, and Dunlap.

The change to daylight adaptation is, however, influential except in the case of Johnson, whose acuity seems to be

exceptional. Tests with the Cobb acuity-object also have shown Johnson to have unusual acuity with darkness adaptation. It is quite probable that the slight effect of the increasing illumination in the darkened room was due to the lessening of adaptation.

The most striking result is the uniform lowness of the threshold. The average range from dark line to bright line lies for the most part near 20″, and is lower in some cases. In the ordinary test object, using object lines, the measurement from fusion to dark line is from 30″ to 60″. The corresponding measurement in the present case is less than 20″; how much less cannot be determined, as it cannot be assumed that either the points of geometrical contiguity, or of physical uniformity of the images, lie half way between the points at which the dark line and the bright line respectively appear. Schuster states that the intensity at the edge of the *geometrical* image of a uniformly bright surface must be "half the intensity observed at some distance inside the edge," because when two surfaces are placed with edges in contact a uniformly illuminated surface is obtained.[1] Assuming this to be true of the physical image, it is not necessarily true of the psychological image, as irradiation and contrast (physiological) effects occur at the margins of the images. The fact that both light line and dark line thresholds tend to shift with darkness adaptation indicates influences of this sort, and we should accordingly expect the light line threshold in general to be nearer the point of geometric image contiguity than is the dark line threshold: an expectation that is justified by the facts.

Table II gives the results of tests to find the effects of lenticular aberrations. My right eye is corrected with a lens of 0.50 C., axis 80°, prism ¾° B.D. Tests were accordingly made on the eye with and without the correcting lens, in the astigmatic axis and at 45° on either side. The results (II, 1, *A*, *B*, and *C*) show that even a low degree of astigmatism is detectible by this means and also that my eye is slightly undercorrected by the lens.

[1] Schuster, 'Theory of Optics,' page 151.

Tests were carried out on both of Martienssen's eyes (II, 2). He preferred to use his left eye in any sort of monocular observation. On being questioned about this he said he had always used that eye because it seemed more natural. The tests seem to indicate a slight degree of astigmatism in the right eye with less in the left. Martienssen's eyes had never been refracted.[1] The degree of astigmatism is not great, for with uncorrected eyes requiring from one to two diopters of cylindrical correction, the instrument cannot be used at all.

Whether the instruments and methods for acuity test above described will be practically useful remains to be seen. In the matter of precision and convenience the apparatus seems superior to devices hitherto in use. The fact that the results differ from those obtained by means of the several other devices is immaterial. Measurements of this kind give comparable results only when the same instruments and methods are used. Since the duoscope method seems sensitive to adaptation changes, it may be possible to use it as an adaptometer; since it seems not sensitive to brightness changes over a considerable range, it may be a useful instrument for practical testing of eyes. It seems especially suitable for detecting slight degrees of astigmatism, and for detecting the accuracy with which lens corrections for astigmatism are made, in experimental work where accurate control of the observer's eye is required.

[1] Since the above was written, Dr. Cobb has refracted Martienssen's eyes, with the following results (sine midriatic):

O.D. –. 37 cyl. axis 175°

O.S. –. 25 sph. –. 37 cyl. axis 155°.

It is probable that the fact that the duoscope readings for the Right Eye are not harmonious (*e. g.*, the inefficiency at 67.5), is due to over accomodation.

AN ELECTRO-MECHANICAL CHRONOSCOPE

BY JOHN W. TODD

University of North Dakota

Provided that certain changes are made in its electro-magnets and that it is skilfully handled the most reliable chronoscope known is the Hipp chronoscope. But for want of skilful handling it is not unusual to see a dust-coated copy of the instrument stored away in a museum for apparatus that looks nice but is rarely used. Nevertheless the Hipp deserves more respect. After having invested in the costly piece with its control-hammer additional, the experimenter should fit it for service by rewinding its electro-magnets with coarser wire, by insuring a steady current with a good gravity battery of 12 cells, by discarding the control-hammer and employing some type of gravity chronometer for control tests.[1] It is the fineness with which the instrument is designed to record times that makes it unsatisfactory in inexperienced hands but that insures reliability when correctly operated.

Even after the corrections indicated are made the instrument must be constantly watched and tested, as a fluctuation of the current or a slight change in the adjustment of the delicate parts of the apparatus may produce a chronoscope variation that will entirely obscure the variations in reaction time. The instrument responds to all irregularities and is never popular when operated in a hit and miss manner. Many attempts have been made to devise a simpler chronoscope than the Hipp. The special aim has been to construct one that will eliminate the constant care of control and minimize

[1] The manner of making these changes and the reasons for them may be found in the *National Academy of Sciences*, 7, 397 ff., 1893 (Cattell and Dolley). After correcting the instrument as indicated these writers found average variable errors for seven series of ten single tests of the chronoscope as follows: 0.96, 0.8, 0.42, 0.4, 0.64, 0.64, and 0.56σ. Using the same instrument corrected by Cattell the present writer in making several thousand reaction tests found an average variation of the chronoscope in control tests of about 1σ ("Reaction to Multiple Stimuli," *Archives of Psychology*, No. 25, 8, 1912).

the possibility of getting out of adjustment. The simplest chronoscope is one so designed as to harness the force of gravity for marking off units of space that may be given time values. This arrangement eliminates delicate clockwork propelling devices and reduces the number of adjustable parts three fourths. Even after the chronoscope is reduced to its simplest terms three difficult problems remain, First, to devise a reliable chronoscope release; Second, to construct a sufficiently accurate reaction recorder, and, Third, the greatest problem of all, *to put down a chronometric scale that is trustworthy.*

The chronoscope described in this article consists of a

FIG. 1.

disc compounded of two adjustable parts (D_1 and D_2, Fig. 1), which are two circular planes of 1/16 in. brass, 11.5 in. in diameter, with a concentric semicircle of each having a radius of 4.75 in. cut away, leaving in each case a marginal area 1 in. in width. One of the discs is constant with respect to a pendulum attached to their common axis while the other is adjustable to allow the various apertures of a tachistoscopic attachment described later in this article. These discs are held rigidly together by a set-screw (*S. sc.*, Fig. 2) and rock with the vibrating pendulum. In making a chronometric reading with the electrical arrangement the initial position of the pendulum, *i. e.*, horizontal, is maintained by the force

of an electro-magnet (*EM*, Fig. 2) upon the armature (A_2) carried near the base of the pendulum whose socket (*P.S.*) is shown. To minimize friction the shaft carrying the discs and the pendulum has cone bearings (Fig. 2). The pendulum weights are two cylinders of lead set in brass. The gross relations of the various parts of the apparatus are shown in the upper left corner of Fig. 1, a lateral photograph of the apparatus.

Fig. 2 is a diagram showing the details of the chronoscope

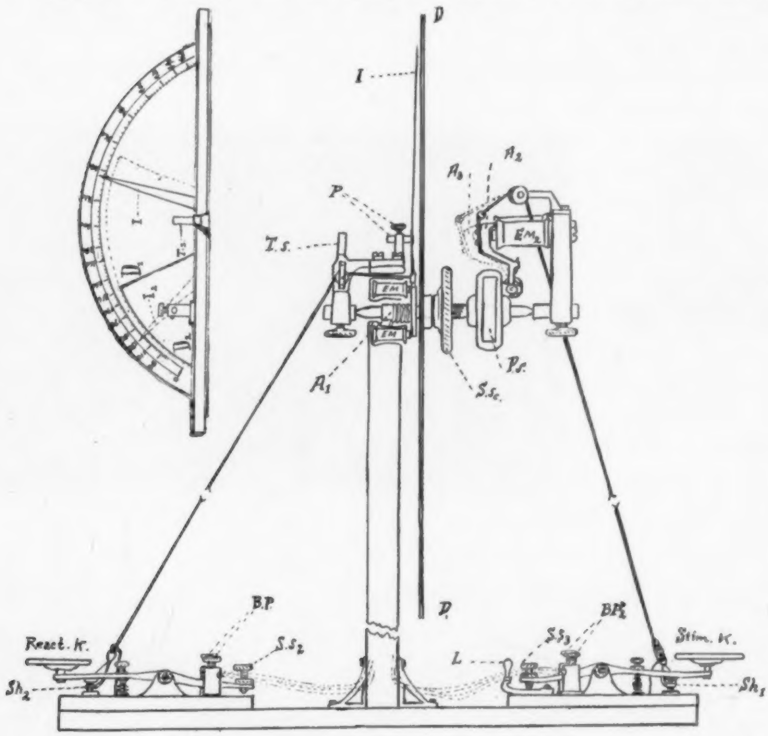


Fig. 2.

arranged for both *mechanical and electrical* stimulus key and reaction key. The figure shows the apparatus set for *mechanical* operation. Sh_1 is a copper shoe held firmly between the contact points of a stimulus key by means of the insertion of the inclined surface of the lever, *L*, and the re-siliency of the key shaft, supporting by means of a cord over a

pulley the armature, A_2, which holds the pendulum in the initial horizontal position. Sh_2 is likewise a copper shoe between the contact points of the reaction key held firmly by the reagent's finger upon the button, and holding the armature (A_1) of the reaction index to its initial position against the arm of the post, *P*. The armature, A_1, to which the reaction index (*I*) is attached moves freely upon the spindle bearing the chronoscope disc, and by means of a coiled spring flies against the disc when release is made and is carried with it allowing the index pointer to escape the arm of the post, *P*, by which it is held during the reaction interval.[1] When the pendulum is in the initial position the index point rests upon the zero point of the chronometric scale. The index may be thrown back to the zero point from any position on the scale by pulling upon the cord to which Sh_2 is fastened.

When the stimulus lever (*L*) is suddenly pulled the stimulus hammer ($S.S_3$) by virtue of the resilient shaft which carries it and the rebound of the strong coiled spring near its fulcrum strikes the solid metallic base a blow emitting a sound which serves as a stimulus, and whose intensity is variable by means of set screws. With the drop of the stimulus hammer upon the metallic base and the emission of the sound the shoe actuated by the strong spring of the release armature flies from between the points, while the armature flies back to position A_3. This releases the pendulum and carries the chronometric scale in the negative direction counting against the reagent until the index flies upon the disc marking the close of the stimulus-reaction period.

The difficulty in attempting to devise a mechanical chronoscope is to give it versatility. It is not hard to provide a release that at the same time serves as a sound stimulus, and that offers no resources for the presentation of touch and light stimuli. With the mechanical device described above, however, it is possible to give all three stimuli. This is shown by Fig. 3, a diagram of the three-stimulus key. The method of giving the sound stimulus is described above.

[1] This type of armature although employed in a somewhat different manner was first used by Bergström in a pendulum chronoscope figured and described in the Psychological Review, VII., 1900, 438 ff.

When it is desired to present a light stimulus with the mechanical chronoscope a light wooden arm long enough to extend beyond the base of the chronoscope is attached to the shaft of the reaction key. This wooden arm carries a small black square (*A*) operating as a shutter to a 1 cm. aperture (*Ap*) in a black screen large enough to conceal the movements

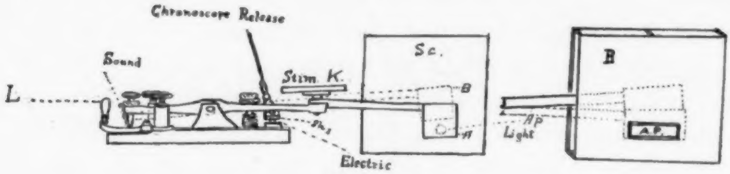


Fig. 3.

of the experimenter. When it is desired to use tactual stimuli the stimulus key, by means of the binding posts, is inserted into the primary circuit of an induction coil preferably with condenser. The reaction key is introduced in the secondary circuit in such manner that the cathode is in the button of the key and the anode in a shallow vessel of salt water. If the vibrating armature of the current interrupter of the induction coil is tied back securely against its adjustable contact and the reacting finger rests upon the cathode and the fingers of the other hand rest in the vessel, a shock is felt in the reacting finger when the primary circuit is broken at Sh_1. As the key is figured above it is set for all three stimuli which would be administered simultaneously with the sudden pull toward the experimenter of lever *L*.

By removing the arrangement for light stimulus the pair of stimuli, sound and shock, may be given together; or by getting out of series with the induction coil, the paired stimuli, sound and light, may be given. Likewise it is possible to eliminate any two stimuli and administer a third singly. When light is presented singly a small rubber plate is attached to the posterior side of the stimulus lever (*L*) to eliminate the sound. In all these cases it is seen that the chronoscope release is mechanical, by means of Sh_1. Fig. 2 by means of dotted lines from the binding posts suggests the wiring arrangement for electrical operation of the chronoscope. When

the current is used the two cords are serviceable to set the pendulum armature and to throw the reaction index back to zero.

It is seen that the mechanical device can be operated only when the experimenter and reagent are at close range. When, however, it is desired to give stimuli from a distance or to have the reaction from another room the electrical arrangement mentioned above must be employed. The reaction movement called for by the apparatus is in all cases of the break-circuit variety. The chronoscope may be put to the same tasks that are attempted by any type of electrical chronoscope, at the same time affording a mechanical arrangement that would seem to meet the objections of those opposed to the electric chronoscope.

Method of Laying the Chronometric Scale

A chronoscope is a device for visualizing intervals of time by freely initiating or terminating the regular movement of either a point along a graduated scale or a graduated scale past a point, each division of the scale being the space traversed in a given unit of time. Many a chronoscope has been devised with perfect balance and bearings but failed because its scale was too largely a matter of speculation. *The chronometric scale is the real chronoscope*—propelling the scale or moving an index uniformly along it are comparatively easy accessories.

The possibility of laying a definite chronometric scale was one of the factors that prompted the present device. Fig. 1 shows the method of deriving the scale and of placing it upon the chronoscope disc. One of a pair of synchronized differential tuning forks of 256 v.d. frequency is loaded with a small aluminum feather (A_1) by means of a stiff wax. It is then sounded with its companion, the number of beats per second counted and its vibration-rate calculated. Then by means of wax another aluminum feather (A_2) is attached to the second fork, and small increments are made to the wax until the beats disappear. The forks are again synchronical and their vibration-rate is that formerly calculated. They are mounted

as shown in Fig. 1 in such manner that feather A_2 touches the smoked drum of a kymograph and feather A_1 rests upon the smoked edge of the chronoscope disc. Into this arrangement a third member is brought, a Morse key rearranged for break-circuit contact, and bearing two aluminum feathers at the end of its shaft beneath the button (*P*). One of these feathers is adjusted to lie within the same radius of the disc as the tuning fork feather, A_1, and the other is squared with the point of the feather upon the kymograph.

After the disc is brought to its initial position and the circuit to the electro-magnet (*EM*) is closed, it is seen that a pressure upon the button (*P*), which short-circuits the current to *EM*, will release the disc. At the same time a mark will be recorded upon the kymograph and another upon the smoked edge of the chronoscope disc. The entire procedure is as follows: Start the forks, and with the hand suddenly revolve the drum of the kymograph, having disconnected it from the clock-work mechanism, and with the other hand press upon the button (*P*) at least twice in quick succession.

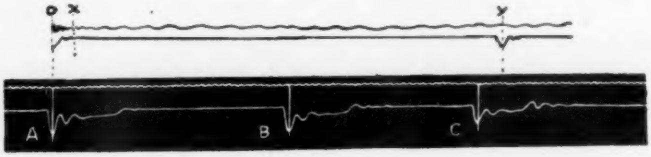


FIG. 4.

Lower Fig. 4 is the kymograph record of the pressures, and upper Fig. 4 is a diagrammatic view of the disc record straightened and brought into relationship with it. The time value of the distance *o-y* on the chronoscope scale is readily counted off from the distance on the kymograph scale, *A-B*. In laying the chronoscope scale it is necessary only to read toward *o* from the point *y* to establish the first distinguishable chronometric value from *o*. There will always be the space *o-x* whose time value *in toto* is known but whose individual waves are too close together to be distinguished.

After setting the chronographic records with shellac the scale of time values is engraved upon the chronoscope disc

at grade points located by producing a radius of the disc through the crest of each tuning-fork wave. The time value of each wave-length was 4^{σ} which save in the case of waves near x is graded in four equal parts, or to the 1^{σ}. This chronographic method of laying the scale is superior to the method of employing sparks produced by an induction coil with a tuning-fork interrupter because the sparks deviate considerably in making the aerial gap, and fail to indicate the true location of the scale.

In order to test the reliability of the chronoscope a control

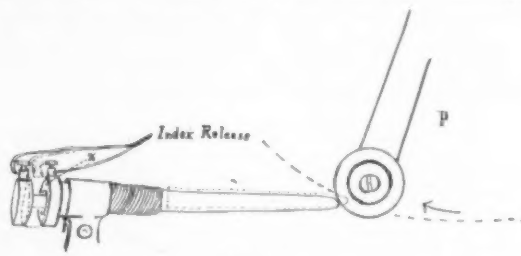


Fig. 5.

apparatus (Fig. 5) is used consisting of a shaft with a polished metal point so poised that its axis if produced is a secant of the pendular arc. This shaft is carried by a comparatively facile spring in a rigid base. When the pendulum strikes the shaft it is pushed back breaking the circuit at x, thus releasing the chronoscope index and recording upon the scale the interval between the release of the pendulum and its striking the shaft. The values given below are the averages of 12 groups of control tests of 10 trials each, or 120 tests. The mechanical release was used in the tests.

Average Interval	Average Variation
519.4^{σ}	0.84^{σ}
520.2	1.08
520.4	0.64
520.1	0.18
520.4	0.64
521.4	0.80
519.5	1.00
519.0	1.00
520.3	0.42
519.8	0.48
519.9	0.72
522.0^{σ} Gross Av. 520.2^{σ}	0.42^{σ} Gross Av. 0.68^{σ}

The present device by means of its adjustable discs (D_1 and D_2, Fig. 1) affords a tachistoscope that is fairly serviceable.[1] The point-exposure time may be read off from the chronometric scale, and affords a maximum point exposure of 420^σ. The stimuli are held by a clip behind the discs and are seen through the sector of the compound disc. *B*, in Fig. 3, is a diagram showing a tachistoscopic attachment for the stimulus key (*Stim. K*) making it possible to expose words, colors, etc., in a rectangular aperture for reaction experiments in discrimination, cognition, choice and association. The screen is large enough to conceal the operations of the experimenter from the reagent. By using the rubber plate under the sound hammer the exposures are made almost noiselessly.

Summary of the Special Features of the Chronoscope

1. It allows either mechanical or electrical release of the time scale, involving in each case the same parts, and making it possible to work where a steady electric current is not available.

2. The reaction mechanism may be operated either mechanically or electrically.

3. The device makes it possible to lay a chronometric scale whose units can be exactly placed to within a short distance of the zero point, and whose total value is exactly known.

4. By means of an attachment time exposures may be made that are measured off on the chronometric scale, and the variety of compound-reaction stimuli can be given.

[1] In his "Mental and Physical Tests—Simpler Processes," 1914, pp. 263 ff., Whipple figures and describes a disc tachistoscope of his own construction and one now commonly in use. In his instrument the point-exposure times are calculated from the relative positions of weights upon the two counterbalancing arms that rotate the disc. In the present arrangement the times are shown on the chronometric scale.

XVIII. PRACTICE IN ASSOCIATING COLOR-NAMES WITH COLORS[1]

BY WARNER BROWN

It has long been known that the process of recognizing and naming a color takes more time than the process of recognizing and naming an isolated printed word, such as the word, for example, which designates the same color.[2] The following experiments represent an attempt to gain a clearer understanding of this phenomenon.

The first hypothesis which presented itself was that words can be recognized and named more rapidly because we have had more practice in doing this than in naming colors.[3] Accordingly a practice experiment was contrived on the basis of Cattell's familiar color-naming test.[4] Experi-

[1] From the Psychological Laboratory of the University of California.

[2] James, W., 'Principles of Psychology,' 1890, Vol. I., p. 559. Cattell, J. McK. 'Ueber die Zeit der Erkennung und Benennung von Schriftzeichen Bildern und Farben, *Philos. Stud.*, Vol. 2, 1885, pp. 635–650.

[3] This explanation of the phenomenon is clearly stated by Cattell in the account of its discovery which he gives under the title, 'The Time it Takes to See and Name Objects' (*Mind*, Vol. 11, 1886, p. 65). He says, "The time was found to be about the same (over ¼ sec.) for colors as for pictures, and about twice as long as for words or letters. Other experiments I have made show that we can recognize a single color or picture in a slightly shorter time than a word or letter, but take longer to name it. This is because in the case of words and letters the association has taken place so often that the process has become automatic, whereas in the case of colors and pictures we must by a voluntary effort choose the name."

The same interpretation is given by J. O. Quantz in his monograph, 'Problems in the Psychology of Reading,' Psychol. Rev. Monog., No. 5, 1897, p. 10. "The association is of the same sort in words as in forms or colors, for the connection between the written symbols and the spoken sound of any given word is just as arbitrary as is that between a particular geometrical form and its name as uttered. But the association between forms or colors and their names, being less necessary than between written and printed (spoken?) words has been less frequently formed and the former has remained a voluntary process while the latter has become automatic through repetition."

[4] Cattell and Farrand, 'Physical and Mental Measurements of Students of Columbia University,' Psychol. Rev., Vol. 3, 1896, p. 642. Wissler, C., 'The Correlation of Mental and Phsyical Tests,' Psychol. Rev. Monog., No. 16, 1901, p. 8. Hollingworth, H. L., 'The Influence of Caffein on Efficiency,' *Arch. of Psychol.*, No. 22, 1912, p. 16.

ence had shown that the Columbia test was weak in the following points: Not all the color names were equally familiar; they were not all equally hard to say (for example *red*, *yellow; blue*, *violet);* there were strong brightness contrasts between some of the colors; the chance arrangement of the colors resulted in some bad sequences; the one-centimeter squares were too small, making it difficult to 'keep the place' with the eye. The test was accordingly modified in these respects: The color squares were increased in size to one inch; the sequence was so arranged that no color square was placed next to another of the same color and a color was not permitted to occur less than twice nor more than three times in any row; only four different colors were used in any one set and these were all either 'light' (white, pink, brown, gray) or 'dark' (black, red, blue, green);[1] the colors all had one-syllable names; all of these names were highly familiar.[2]

It was expected on the hypothesis of Cattell and Quantz that sufficient practice would make it possible to read off the color names as rapidly from the colors themselves as from a printed list. If the difference in speed depends upon previous practice it should, by further practice, be possible to reduce the time consumed in reading colors but not possible to reduce to any considerable extent the time required to read a list of words. In order to test the truth of this hypothesis it was necessary to show not only that the speed of color naming can be increased by practice but also that the speed of reading words can *not* be increased so much by an equal amount of practice. For the practice in reading words, lists were typewritten with the one hundred color-names arranged in the same order as the colors themselves. The words in

[1] The colors used were the papers supplied by the Milton Bradley Company, of Springfield, Mass., under the following designations: Black, White, Neutral Gray No. 2, Engine Colored Paper No. 2B (brown) and No. 1B (pink), Red, Green, and Blue.

[2] The modified form of the Columbia test recommended by Woodworth and Wells, 'Association Tests,' Psychol. Rev. Monog., No. 57, 1911, p. 49, meets most of the difficulties mentioned above, but unfortunately it was not published until after the present experiments were partly completed. It may be noted that in the Woodworth and Wells test the colors appear on a white background whereas in the form here used the squares were larger and juxtaposed without background.

each line were separated by a comma and one space; the lines were separated by a triple space. For each set of colors there were, of course, four distinct lists of words, corresponding to the four arrangements of colors which were encountered on beginning in the four different corners of the color-set. For every practice trial in associating the colors with their names there was a practice in reading the words from the corresponding list.

A record-blank, including complete directions, was given to each worker at each practice sitting; it read as follows:

Directions for the Experiment on Naming Colors

There are two boards of colors. Each board contains 25 squares of each of 4 colors, and there is a different color in each corner of the board. There are 4 typewritten lists of colors for each of the boards, and each list begins with the name of the color in one corner of the board, and gives the names of the colors in the order of their appearance on the board.

The purpose of the experiment is to measure the maximum rate of speaking when reading the lists of words or naming the colors, and to see how much this rate can be increased by practice.

First day's work. Take the time with a stop-watch for reading aloud, as fast as you possibly can, the words on the typewritten list beginning with Black. Enter the time, in seconds and fifths of a second, opposite "List black" in the table below. Then take the time for calling out the names of the colors, as fast as you possibly can, from the board, beginning with Black in the upper left-hand corner and reading by rows from left to right. Enter the time opposite 'Board black' in the table. Then enter the time for each of the remaining items in the table, being careful to take them in the order indicated by the numbers.

1. List black...........	3. List white...........
2. Board black.........	4. Board white.........
5. List blue............	7. List brown..........
6. Board blue..........	8. Board brown.........
9. List green...........	11. List pink............
10. Board green.........	12. Board pink..........
13. List red............	15. List gray............
14. Board red...........	16. Board gray..........

Second and succeeding days. Use only one board of colors and the lists which belong with it. Do not look at the other board or its lists, nor allow any one to read them in your hearing. Record the times for the right (left)[1] hand half of the table in the order given, and do nothing with the other half of the table.

Twelfth day. Exactly the same as the first day.

Forty-five students took part in the experiment. All

[1] If the subject was to practice the 'dark' colors the word *right* was expunged; if he was to practice the 'light' colors the word *left* was expunged.

practiced for twelve practice-periods. Most of them worked twice a week, but a few practiced daily. Twenty-five of the forty-five were women. Twenty, of whom ten were women, practiced on the 'dark' colors. Twenty-five, of whom fifteen were women, practiced on the 'light' colors. As no essential difference appears between the light and dark colors the data have been combined for the entire forty-five workers.[1]

The condensed data are presented in Table I. The table

TABLE I

GAIN BY PRACTICE IN NAMING COLORS AND READING WORDS

Average of 45 Subjects

The time is the average of the 4 trials made each day.

Day.	Colors: Av. Time Required to Name Them, Secs.	Colors: Av. Gain in Speed Over 1st Day, Secs.	Colors: Av. Gain in Speed Over 1st Day, Per Cent.	Words: Av. Time Required to Read Them, Secs.	Words: Av. Gain in Speed Over 1st Day, Secs.	Words: Av. Gain in Speed Over 1st Day, Per Cent.	Ratio: Time for Colors Divided by Time for Words
1	55.8			35.2			1.59
2	50.9	4.9	8.8	33.0	2.2	6.3	1.54
3	46.4	9.4	16.8	31.6	3.6	10.2	1.47
4	45.2	10.6	19.0	30.8	4.4	12.5	1.46
5	43.7	12.1	21.7	30.2	5.0	14.2	1.44
6	42.8	13.0	23.2	30.4	4.8	13.6	1.41
7	42.4	13.4	24.0	29.9	5.3	15.1	1.42
8	41.4	14.4	25.8	29.5	5.7	16.2	1.40
9	41.4	14.4	25.8	29.4	5.8	16.5	1.41
10	41.1	14.7	26.4	29.0	6.2	17.6	1.42
11	40.7	15.1	27.1	29.4	5.8	16.5	1.38
12	41.4	14.4	25.8	29.3	5.9	16.8	1.41

shows the average time required by the 45 subjects for naming the 100 colors and for reading aloud the 100 words. The time is the average for the four trials which were made each day.[2]

[1] On the first day of work, when records were made for all of the subjects with both light and dark sets (*i. e.*, the first practice record with one set and the first check record with the other set) the times were as follows:

Time required to name 100 dark colors 56.0 sec.; 100 'dark' words 36.0 sec.

Time required to name 100 light colors 55.8 sec.; 'light' words 35.2 sec.

This insignificant advantage of the light sets remains unchanged through the course of practice. Most persons prefer to work with the light colors on esthetic grounds. Some subjects complain of getting the tongue twisted around the words, *blue* and *black* in the dark sets because of the identity of their initial sounds.

[2] These four trials did not differ greatly from one another. As a rule the first trial was better than the others except that on the first day, and to some extent on the

The practice gains are shown both in seconds and in per cent. In both cases the amount of gain is computed on the basis of the speed on the first day of work. The table further shows the ratio between the time required for colors and the time required for words.

In Table I*a* the records are shown for the tests which

Table I*a*

Tests on Unpracticed Sets, for Which Records were Made on the 1st and 12th Days of Practice

Column headings as above.

1	55.9			35.8			1.56
12	47.4	8.5	15.2	32.0	3,8	10.6	1.48

Table I*b*

Separate Statement for Men and for Women for the 1st and 12th Days of the Regular Practice Work

Figures for Women in Italics

Headings as above.

1	58.9			35.6			1.66
1	*53.3*			*35.1*			*1.52*
12	42.0	16.9	28.7	29.9	5.7	16.0	1.40
12	*39.9*	*13.4*	*25.1*	*29.0*	*6.1*	*1.74*	*1.38*

were made on the first and last days with different sets of colors and words.

In Table I*b* the data of the first and last days are arranged to display the fact that women excel men in speed in naming colors, but that men improve more with practice.[1]

From the data of Table I. and from an inspection of the curves of Fig. 1 it can be seen that the hypothesis on which this experiment was based is probably not true. At the end

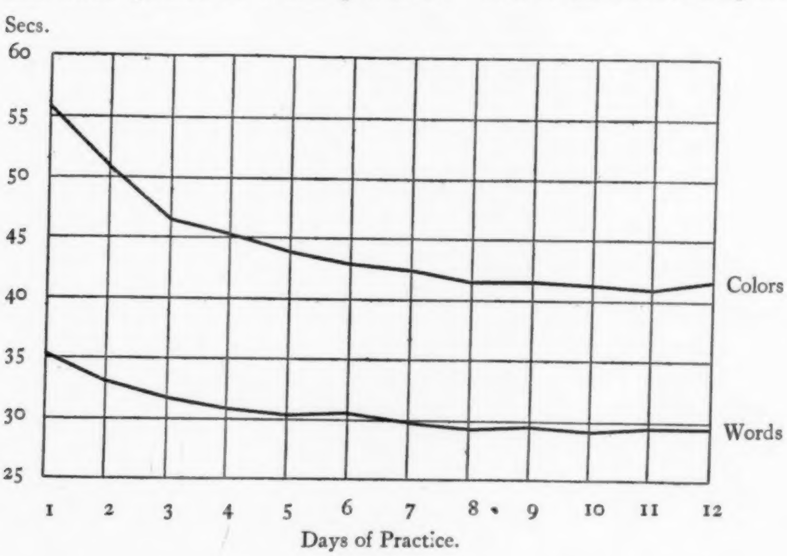
second day, there was improvement from trial to trial. The following figures were obtained by averaging the records for the last ten days of practice:

	1st trial	2d trial	3d trial	4th trial
Time for 100 colors..........	42.3	42.3	43.0	42.8
Time for 100 words..........	29.0	30.4	29.9	30.5

Evidently the practice gains during this period occur in the intervals between sittings, 'overnight,' and not during the course of a sitting.

[1] The superiority of women in naming colors has been observed by Woodworth and Wells, Psychol. Rev. Monog., No. 57, 1911, p. 51, and by Wissler, Psychol. Rev. Monog., No. 16, 1901, p. 17.

of twelve periods of practice it is evident that only a very slight further increase of speed in naming colors can be anticipated, no matter how much more practice is taken; yet the absolute rate in naming colors remains much slower than the rate of reading the same words from the list and is even slower than the word rate was before the beginning of practice. Furthermore the life-long practice which we have had in reading words has not brought that function to a maximum speed; on the contrary it shows an amount of practice-improvement almost proportional to the improvement shown in naming the colors. For every second gained in naming colors at any stage of practice approximately half a second has been gained in reading words. The ratio between speed



in color naming and speed in word reading (the last column of Table I.) shows no indication of approaching unity.[1]

From these data it seems safe to conclude that the dif-

[1] The statements of this paragraph are true not only for the average results given in the table, but for each individual subject who took part in the experiment. No statement of the variability or probable error of the measurements has been made because such a statement could have no direct bearing upon the interpretation of the figures in the present connection. The individual differences in absolute speed were very large, but they do not in any way affect the results.

ference in speed between color-naming and word-reading does not depend upon practice.

Further confirmation of this conclusion is found in the fact that the effects of training in reading words are specific for the particular words read and do not extend to other words. It will be recalled that each person was trained upon either the 'light' or the 'dark' set, but that a test was made, at the first and last sitting, of his speed with the other set (the one he did not practice). The results of these tests are indicated in Table I*a*. The speed on the unpracticed sets at the end of twelve days of practice is better than on the practiced sets on the second day of practice, but not so good as on the third day. In other words, three days of direct practice are better than two days of direct practice plus ten intervening days of indirect practice. This, too, in a case where the conditions regarding eye-movement and general adaptation to work might lead us to anticipate a considerable amount of transference of practice or 'formal' training. In the present connection the significant fact is that the amount of transfered practice is but little greater in the case of reading words than in the case of naming colors. Apparently we must have practice in reading specific words before we can attain great proficiency in reading them. It can not, therefore, be safely asserted that we read color names faster than we name colors simply because of the large amount of practice which we have had in reading words in general.

The Second Experiment

It now seemed clear that the effects of previous practice do not afford a sufficient explanation of the difference in speed between color-naming and word-reading. Accordingly the problem was attacked from another quarter. The introspections of practically all of the students who had taken part in the first experiment agreed upon one point: It is easier to speak a printed word than to name a color because when you want to name a color you have first to think of the name (the word) and then speak it, whereas the printed word can

be uttered without your having to think of anything. The observations of our foreign-born students were particularly clear on this point.[1]

On the basis of these introspections the hypothesis was formed that the process of color-naming would be facilitated by suggesting the word at the moment the color was presented. For actual experiment color-sets were prepared which had entire words or parts of words printed on the face of the colors themselves.

Nineteen students finally completed all the stages of this experiment. They were first given fifteen periods, twice a week, of practice in reading the lists and naming the colors from sets upon which nothing was printed, just as in the previous experiment. The color-set was named over three times at each sitting and the list of words was read once.

TABLE II

Day	Time Required to Name 100 Colors, Secs.	Time Required to Read 100 Words, Secs.	Ratio: Time for Colors Divided by Time for Words
1	53.8	35.5	1.52
2	48.2	32.7	1.47
3	46.2	31.7	1.45
4	45.3	31.1	1.46
5	42.9	29.8	1.44
6	42.2	30.2	1.39
7	41.3	29.4	1.41
8	40.7	28.8	1.41
9	39.3	28.5	1.38
10	39.5	29.0	1.36
11	40.4	28.8	1.40
12	38.8	27.6	1.41
13	38.2	27.7	1.38
14	37.9	27.6	1.37
15	37.1	27.4	1.35

Only the 'light' set was used. The words of the list, instead of being printed in a regular list with the ten words of a line separated by commas, were now typewritten on separate squares of paper, one inch square, which were mounted on a board just as the color-squares were mounted, so that the eye-movements involved were as nearly as possible the same as for the colors. The data for these fifteen preliminary

[1] Three Japanese and one Armenian took part in the experiment, but their records are not included in the tabulations.

practice sittings are given in Table II. The figures agree substantially with those presented in Table I.[1]

After the preliminary practice, which was only intended to bring the students to such a point that their speed for simple colors and lists of words would be nearly uniform from day to day, experiments were begun with sets of colors arranged just like the others except for words or letters typewritten upon them. The following transcript of the directions gives a sufficient outline of the course of this experiment.

DIRECTIONS

Sixteenth day. Read the list of words beginning with *brown:* then read the simple color-set beginning with *gray.* Then read color-set 2 with *b* on brown; then set 3 with *w* on white; then set 6, *b* on brown, *w* on white, *p* on pink, and *g* on gray.

Seventeenth day. Read the list of words beginning with *gray:* then the simple color-set beginning with *pink.* Then read color-set 7, *gr* on gray; then set 4, *p* on pink; then set 10, *br* on brown, *wh* on white, *gr* on gray, and *p* on pink.

Eighteenth day. Read the list of words beginning with *brown:* then the simple color-set beginning with *gray.* Then read color-set 11, *own* on brown; then set 12, *ink* on pink; then set 15, *own* on brown, *ink* on pink, *ite* on white, and *ay* on gray.

Nineteenth day. Read the list of words beginning with *gray:* then the simple color-set beginning with *pink.* Then read color-set 16, with full words on all colors. Then read the simple color-set again beginning with *brown.* Then read color-set 16, full words, again.

Twentieth day. Read the list of words beginning with *pink:* then the simple color-set beginning with *gray.* Then read color-set 16, with full words on all colors, two times. Then read the simple color-set beginning with *brown.*

Twenty-first day. Read the list of words beginning with *brown:* then the simple color-set beginning with *gray.* Then read color-set 13, with *ite* on white; then set 14, with *ay* on gray; then set 15, *own* on brown, *ite* on white, *ink* on pink, and *ay* on gray.

Twenty-second day. Read the list of words beginning with *gray:* then the simple color-set beginning with *pink.* Then read color-set 8, with *br* on brown; then set 9, with *wh* on white; then set 10, *br* on brown, *gr* on gray, *wh* on white, and *p* on pink.

Twenty-third day. Read the list of words beginning with *brown:* then the simple color-set beginning with *gray.* Then read color-set 4, with *p* on pink; then set 5 with *g* on gray; then set 6, with *b* on brown, *p* on pink, *w* on white, and *g* on gray.

The data for the last eight days of this experiment are presented in Table III. They are combined in the table so that wherever two records of the same kind were obtained on

[1] It may be noted that the rate of improvement is here almost the same as in the earlier experiment in spite of the fact that the colors were practiced only three times and the words only once instead of four times as in the earlier experiment. In view of the fact already mentioned that the first trial of a sitting is usually the best there is reason for believing that nearly the same results could be obtained in this work by one trial per day as by four trials per day.

Table III

Time Required to Name 100 Colors, to Read 100 Words, and to Name 100 Colors with the Help of Printed Cues

Day	Simple Colors	Words	Colors on Which the Following Letters Were Printed as Cues to Help in Naming the Colors	
16	38.1	28.1	40.8	An initial consonant on one color.
			39.4	An initial consonant on each color.
17	36.6	27.6	38.8	Initial pair of consonants on one color.
			36.4	Initial pair of consonants on each color.
18	36.6	27.6	38.0	Vowel and final consonant on one color.
			38.4	Vowel and final consonant on each color.
19	36.1	27.6	28.6	Entire word on each color.
20	35.7	27.2	28.3	Entire word on each color.
21	36.1	27.2	37.3	Vowel and final consonant on one color.
			36.4	Vowel and final consonant on each color.
22	35.0	27.9	36.9	Initial pair of consonants on one color.
			32.3	Initial pair of consonants on each color.
23	35.3	27.9	35.2	Initial consonant on one color.
			34.6	Initial consonant on each color.

the same day only their average appears. When the entire words are printed on the colors it is possible to read the words without attending to the colors, but even in that case the average speed is not so great as when the words are read alone without the colored background, as may be seen in the records of days nineteen and twenty. After having practiced with the full words on the colored backgrounds some of the students found it possible to read the color-names directly upon seeing the initial letters without considering the background. This accounts for the fact that the records for the twenty-second and twenty-third days, with initials, are considerably better than the records for the sixteenth and seventeenth days under the same conditions.

From the results of this part of the experiment it may be concluded that the association process in naming simple objects like colors is radically different from the association process in reading printed words. The presence of a visual symbol of the sound does not greatly, if at all, facilitate the process of association between color and color-name. Phonetic symbols which might suggest the name of the color do not help us in naming it unless they are so clear that they enable us to read the name itself directly without going through the process of naming the color. The one association

process does not reinforce the other. The introspections of all the subjects confirm the figures in declaring that the letters printed on the colors do not serve as helpful cues or prompts, but on the contrary actually interfere with the process of association.[1]

Conclusion

The conclusions of these experiments seem to be entirely negative. No facts have been adduced to explain why more time is required to associate speech movements with a color than with the corresponding printed word. But the evidence does throw some light on the problem in so far as it eliminates very definitely two lines of explanation which have been thought possible. First, the phenomenon does not spring from a difference in the amount of practice which the two functions have had in the past. Second the process of reading words is not involved in the process of naming colors as a subsidiary function. The two functions do not overlap, and in all probability they depend upon distinct physiological processes.

[1] A very similar problem has been attacked with the chronoscope by Bourdon. "Sur le temps nécessair pour nommer les nombres," *Rev. Philos.*, Vol. 65, 1908, p. 426. He finds that the time required to perceive and name a number of points of light (not exceeding four) is only slightly greater than the time required to read arabic numerals. Accordingly he infers that the process of perceiving a few points as a number is as simple as perceiving the symbol of the number. Apparent conflicts between this observation and the results in the case of naming colors are now under investigation in this laboratory.

XIX. THE APPARENT RATE OF LIGHT SUCCESSION AS COMPARED WITH SOUND SUCCESSION[1]

BY BERTHA VON DER NIENBURG

It has often been observed that we perceive a duration marked off by lights as shorter than an identical duration marked off by sounds, a result readily explained by the presence of after-images in the case of the light sensations. Preliminary experiments with series of lights and of sounds indicated, however, that not infrequently the light rate seemed *slower* than the sound rate.[2] This study was undertaken to look into the subject more thoroughly, first from a descriptive view point and later from a causal point of view.

The experiments were conducted during the period from September, 1910, to May, 1911, in the psychological laboratory of the University of California. The subjects were taken from the class in general psychology; their number varied for the several parts of the work.

I. In the first group of experiments the light succession and the sound succession were of equal rapidity. The apparatus in the main consisted of a metronome and a minature electric light, a telegraph sounder, and the necessary switches and connections. The current flowing through the metronome, which was placed in a distant room so that its ticking

[1] From the Psychological Laboratory of the University of California.

[2] Experiments were tried upon a class in psychology at the University of California, a sound-series and a light series of equal rate (240 a minute) being given to all the students together, who thereupon reported their independent judgments in writing. In a first experiment the class was left in ignorance as to the relative rates of the two series, the questions being in the form: "Are the two series of equal rate? If not, which is faster?" The judgments were;

That the light-series was faster (L.F.) 24
That the two series were equal (E.) 55
That the light-series was slower (L.S.) 50

A fortnight later the same class was told that the two series were of equal rate, and the students were asked to tell how the series *seemed* in this respect—whether they seemed equal, or, if not, which seemed faster. The judgments were,

L.F. 41 E. 10. L.S. 70.

might not be heard by the subject, could be sent either into the telegraph sounder, thus marking off the intervals by sound, or into the incandescent electric bulb, when the duration was marked off by light. The entire apparatus was enclosed by screens, so that no light could reach the eye save indirectly through a small aperture (¼ inch in diameter) which flashed the light upon a screen.

The subject, who was seated before the screen upon which the flashes appeared, was told that he would be given a series of taps to be followed by a series of flashes, and that he was to compare the rate with which the taps were coming and the rate with which the flashes appeared. He was allowed a trial in the beginning to insure a perfect understanding of the experiment.

Eighteen subjects, ten women and eight men, were experimented upon. Each subject was allowed to form his judgments in whatever manner seemed most natural to him.

Three rates were used, namely 61, 154, and 183 impressions per minute, or each interval was approximatly 1″, .43″, and .32″ in length. The series were arranged according to the following plan. First, 30 taps at .43″ following 30 flashes at the same rate were given, and the judgment was recorded. This was repeated until 10 judgments had been obtained. The order was then reversed, 30 flashes being given first, with judgment. Next came 10 taps followed by 10 flashes, and a reversal; and 20 taps following 20 flashes, and then the order reversed. This procedure was then followed for the other two rates.

The percentages of respective judgments for the eighteen people, combining, at first, all rates, lengths, and orders, are as follows:

Fourteen subjects were used for one hour only, and while 60 judgments from each was the aim, yet because of irregularities in the apparatus and in the subjects themselves the judgments differed in number from 30 to 100. Four other subjects gave four hours and passed from 180 to 230 judgments. I have grouped these subjects into three groups: those passing from 30 to 50 judgments, those passing from 60 to 100 judgments

'Light faster'	'Light equal to sound'	'Light slower'
37.5%	54.9%	7.6%

NUMBER AND PERCENTAGE OF RESPECTIVE JUDGMENTS PER INDIVIDUAL

Subjects	Total Number of Judgments Passed	'Light Faster' Judgments		'Light and Sound Equal' Judgments		'Light Slower' Judgments	
		Number	Per Cent.	Number	Per Cent.	Number	Per Cent.
Men							
A	200	151	75.5	49	24.5	—	—
B	30	18	60.0	9	30.0	3	10
E	100	48	48.0	50	50.0	2	—
L	230	46	20.0	184	80.0	—	11.7
N	60	25	41.7	28	46.7	7	—
P	40	4	10.0	36	90.0	—	10
S	40	19	47.5	17	42.5	4	—
T	210	115	54.8	79	37.6	16	7.6
Sub-totals and average per cents	910	426	44.7	452	50.2	32	5.2
Percentage of sum of each class of judgments, of total judgments			46.8		49.7		3.5

NUMBER AND PERCENTAGE OF RESPECTIVE JUDGMENTS PER INDIVIDUAL

Subjects	Total Number of Judgments Passed	'Light Faster' Judgments		'Light and Sound Equal' Judgments		'Light Slower' Judgments	
		Number	Per Cent.	Number	Per Cent.	Number	Per Cent.
Women							
C	40	14	35.0	26	65.0	—	—
D	60	32	53.3	27	45.0	1	1.7
F	180	2	1.1	178	98.9	—	—
G	60	25	41.7	27	45.0	8	13.3
H	40	10	25.0	23	57.5	7	17.5
I	80	12	15.0	67	83.8	1	1.3
K	40	14	35.0	15	37.5	11	27.5
M	60	38	63.3	1	1.7	21	35.0
O	80	6	7.5	74	92.5	—	—
R	50	23	46.0	27	54.0	—	—
Sub-totals and average per cents	690	176	32.3	465	58.1	49	9.6
Percentage of each class of judgments, of total judgments			25.5		67.4		7.1
Grand totals and average per cents for all 18 observers	1,600	602	38.5	917	54.1	81	7.4
Percentage of sum of each class of judgments, of total judgments			37.6		57.3		5.1

and those passing from 180 to 230 judgments and giving equal weight to each individual's results we have the following table:

Judgments	No. of Observers	L. F.	E.	L. S.
30 to 50	7	36.9%	53.8%	9.3%
60 to 100	7	38.64%	52.08%	9.28%
180 to 230	4	36.46%	61.6%	1.94%

It will be noticed that there is very little difference in the results of the first two groups. The difference in the results in the third groups I judge to be due to the personnel of the group. I think that from these figures I may conclude that the number of judgments does not noticeably affect the final results and that it is therefore not distorting the facts to throw these individuals together.

With two exceptions each individual varied to a large extent in his successive estimations. With no person was the sound declared faster for a majority of the judgments, while in the case of seven persons it was never considered faster at all. In the case of twelve of the observers, the greater number (the 'plurality') of their judgments were of equality, and with the remaining six observers, the light rate of succession was deemed faster than the sound rate.

II. To obtain a numerical evaluation of the differences in the apparent ratings of the succession of these series, the apparatus was now arranged so that the rate of the flashes could be altered at will. While the sounder was still operated through the metronome, the electric bulb was put on another circuit. The physical light was now continuous; but by revolving on a kymograph before it a wheel from whose circumference eighteen acute angled notches were cut, the effect of flashes was given to the subject who saw the light through a small aperture in a black screen. A piece of ground glass, placed directly at the back of the wheel, served to make the light more distinct. By means of the kymograph, the rate of succession could be varied at will. In this part of the work the duration of each flash of light was the same as that of the dark interval which followed it. The sound rate remained constant, that, is 154 beats per minute. The light rate was varied, by steps of eight beats per minute, between the limits of the variable judgments, which proved to be between 110 and 166 beats per minute. Twenty clicks

followed by twenty flashes were given throughout this group. The light succession first compared was at a rate of 110 a minute, then 126, and so to 142, 158, 118, 134, 150, and 166. Ten judgments for each series were obtained. Five subjects designated alphabetically in the following table, were used, the first four persons having been subjects in the previous work.

The approximate points at which the succession of lights were judged to be of equal rapidity with the 154-beat-per-minute sound-rate were:

Person	
A	134
B	138
C	138
D	126
E	146

In each of these cases, therefore, the light rate was estimated to be (relatively to the sounds) faster than it really was. In the former part of the experiment, where equal rates of succession were judged, the results for such of these subjects as there acted as observers read for the 154-impressions-a-second rate:

	L. F.[1]	E.[1]	L. S.[1]
A	95%	5%	—
B	2.5%	97.5%	—
C	5%	95%	—
D	17.5%	62.5%	20%

Where the two series to be compared were kept equal, the judgments do not show the same uniformity as in those series where the rate of the light succession was varied.

In the entire group of experiments it was evident that there were variations in the judgments of the same individual from day to day, and from individual to individual.

III. *A.* Attention was now given to the various factors that might cause some of the variations in the judgments passed upon the equal rate of succession. The influence of

[1] These abbreviations stand here and elsewhere for judgments 'Light Faster than Sound,' 'Light Equal to Sound,' and 'Light Slower than Sound.'

the rate itself was first observed. Three rates were used, 183, 154, and 61 beats per minute; *i. e.*, each double phase was approximately 0.32″, 0.43″, and 1″ long. From 20 to 120 judgments were given for each rate of six subjects. The percentage of the respective judgments for all are given in the accompanying table:

Number and Per Cent. of Judgments for Varying Rates

Subject	Rates: 61						154						183					
	Judgments of																	
	L. F.		E.		L. S.		L. F.		E.		L. S.		L. F.		E.		L. S.	
	No.	%	No.	%	No.	%	No.	%	No.	%	No.	%	No.	%	No.	%	No.	%
A..	57	71.3	23	28.8	-	—	38	95	2	5	-	—	56	70	24	30	-	—
E..	7	35	11	55	2	10	32	53.3	28	46.7	-	—	9	45	11	55	-	—
F...	—	—	20	100	-	—	1	2.5	39	97.5	-	—	1	0.8	119	99.2	-	—
I...	—	—	19	55	1	5	9	22.5	31	77.5	-	—	3	15	17	85	-	—
L...	—	—	40	100	-	—	3	5	57	95	-	—	43	35.8	77	64.2	-	—
T..	26	37.1	41	58.6	3	4.3	7	17.5	25	62.5	8	20	82	82	13	13	5	5
	90	23.9	154	72.9	6	3.2	90	32.6	182	64.0	8	3.3	194	41.5	261	57.7	5	.8

These results would indicate that the light appears relatively faster as the rate of succession is increased. Individual differences, however, exist.

B. The influence of different lengths of the series was now tested. Three series, of 30 beats, and 20 beats, and 10 beats, respectively, were used. The percentages are made up of from 20 to 120 judgments by eight individuals for each series.

Individual Tabulation of Percentage of Respective Judgments for Series of Varying Lengths.

Subject	Length of Series																	
	10						20						30					
	Judgments of																	
	L. F.		E.		L. S.		L. F.		E.		L. S.		L. F.		E.		L. S.	
	No.	%	No.	%	No.	%	No.	%	No.	%	No.	%	No.	%	No.	%	No.	%
A..	93	77.5	27	22.5	-	—	46	76.7	14	23.3	—	—	12	60	8	40	-	—
F...	—	—	40	100	-	—	1	1	99	99	—	—	1	2.5	39	97.5	-	—
G..	5	25	11	50	4	20	11	55	9	45	—	—	13	65	7	35	-	—
I...	3	75	36	90	1	2.5	6	30	14	70	—	—	3	15	17	85	-	—
H..	7	8.75	73	91.2	-	—	24	24	76	76	—	—	15	37.5	25	62.5	-	—
N..	6	30	12	60	2	10	13	65	7	35	—	—	12	60	7	35	1	5
O..	1	2.5	39	97.5	-	—	3	30	7	70	—	—	35	87.5	5	12.5	-	—
T..	40	44.4	48	53.3	2	2.2	58	58	28	28	14	14	17	85	3	15	-	—
		24.5		71.2		4.3		42.4		55.8		1.8		51.6		47.8		.6

There is here a tendency for the apparent rate of light succession to increase as the series is lengthened. This is not only manifested in the percentages of the eight subjects together, but in those for each individual save one. All subjects agreed that the thirty-beat series was unnecessarily long. Those persons who formed their judgments immediately felt that the ten-beat series allowed sufficient time to make the judgments, while those who formed no estimation until the final beat preferred the twenty-beat series.

C. Since it was considered by many of the observers to be much easier to form judgments when the sound succession was given first, the effect of the order of the light and sound successions was next considered. The same eighteen subjects were experimented upon with the following results:

Per Cent. Distribution of Judgments

Sound-Light			Light-Sound		
L. F.	E.	L. S.	L. F.	E.	L. S.
38	51	11	42	52	6

These combined results indicate a slight increase in the 'light faster' judgments when the order is reversed from sound-light to light-sound. But taking into account the variation in individual cases, we may conclude that while the order may increase the ease with which the judgments can be made, its effect upon the quality of the judgment is slight.

D. All the observers felt that it was easier to form an idea of the sound intervals than of the light intervals, and gave as their reason, that in the latter the flashes disappeared gradually, instead of sharply as the sound did. By raising and lowering the light before the notched wheel described on page 59 the ratio of light to darkness could be changed. The effect of this change upon the rate at which the light succession was judged to be of the same rapidity as the sound succession was studied in experiments upon four persons.

First of all, with the duration of the flash equal to that of the dark phase, and with the sound-series preceding at a rate of 154 beats per minute—there was found for each individual the rate at which the light-series seemed equal to that of the

sound series. While no point was considered the 'equality' consistently by any person, yet one rate was judged to be so more often than others. This rate I shall designate as the 'point of apparent equality.' The ratio of the light to the total cycle of light-phase plus dark-phase was then increased to 75 per cent., and ten judgments made; and then decreased to 25 per cent., and ten judgments recorded; and then back to 50 per cent. The order of these shifts from one ratio to another was inconstant, so that sometimes we would go from 50 per cent. to 25 per cent. and then to 75 per cent., or again from 25 per cent. through 50 per cent. to 75 per cent., and so on. For the various persons the results were as follows:

Subject	Apparent Equality Point of Light Rate When Sound Rate Equals 154[1]	Ratio of Light to Light-plus-Dark								
		25 Per Cent.			50 Per Cent.			75 Per Cent.		
		L. F.	E.	L. S.	L. F.	E.	L. S.	L. F.	E.	L. S.
A ..	128	76	18	6(i)	8.0	78	14	20	58	22(d)[2]
B ..	148	24	24	72(d)	16	78	6	—	—	100(d)
C ..	134	4	24	72(d)	2	76	22	60	20	20(i)
D ..	150	76	24	—(i)	16	78	6	22	38	40(d)

A shows a slight tendency to increase his 'light faster' and 'light slower' judgments at the expense of the equality judgments in the 75 per cent. ratio, while in the 25 per cent. ratio there is a marked increase in the 'light faster' judgments. *B* shows a great increase of judgments 'light slower' at the 75 per cent. ratio, and nearly as great a one for the 25 per cent. *C* shows a like tendency for the 25 per cent. ratio, but exhibits just the opposite tendency for the 75 per cent. ratio. *D* shows an increase of 'light slower' judgments for 75 per cent. and of 'light faster' judgments at 25 per cent. In three cases, therefore, and contrary to expectation (since the interval between flashes is now decreased) the 75 per cent. ratio of light to darkness *decreases* the apparent rapidity of the light, and the 25 per cent. ratio *increases* it.

E. After a month's time, the same observers together with a new one, *E*, were used as subjects to see whether the ten-

[1] Here and in the following table, 'd' indicates decrease in the apparent rate, 'i' increase and 'N' no change.

[2] Determined afresh for this portion of the experiment.

dencies displayed above would exist when the gradation method was used. The work was conducted in the manner described above;[1] that is, with the sound rate at 154, the light rate was varied by steps of eight beats per minute from 110 beats to 166 beats. This was done for each of the three ratios of light to dark, ten estimations being recorded for each rate compared. The approximate points where the light rate was judged equal to the sound rate were:

RATE OF LIGHTS PER MINUTE WHICH SEEMED EQUAL TO 154 SOUNDS PER MINUTE

Subjects	Ratio of Light to L. + D. 25 Per Cent.	Ratio of Light to L. + D. 50 Per Cent.	Ratio of Light to L. + D. 75 Per Cent.
A.........	134 (N) (i)[2]	134	132 (I?) (d?)
B.........	130 (I) (d)	138	118 (I) (d)
C.........	136 (I?) (d)	138	126 (I) (i)
D.........	130 (D) (i)	126	137 (D) (d)
E.........	142 (I)	146	136 (I)

A's point where the light rate seems to equal the 154 sound rate is somewhat higher than it was in the earlier group of experiments; he no longer exhibits the same tendency as before, that is, to regard the light rate as faster for the 25 per cent. ratio than for the other ratios. *B*'s equality point is the same as before, but now the light succession appears to come much faster for both the 75 per cent. and 25 per cent. ratios, which is just the opposite of the earlier results. *C*'s equality point now is somewhat lighter. His judgments are consistent for the 75 per cent. ratio with those of a month earlier, but not for the 25 per cent. ratio. *D*'s point of equality has fallen fourteen beats per minute, and instead of an increased apparent light rate for 25 per cent. as at first, it is decreased. To the new subject, the apparent light rate is increased by the greater ratio of light, and to a less degree is increased by the smaller ratio.

In most cases, then, the 75 per cent. ratio has apparently quickened the light rate, and the 25 per cent. ratio has also quickened it. In the previous group of experiments, the apparent rate was increased or decreased for the 25 per cent.

[1] See page 60.

[2] The lower-case letters indicate, 'increase' and 'decrease' respectively, in the earlier groups (see p. (63)).

and the 75 per cent. ratio about indifferently. The conclusion seems warranted that the judgment of the rate is not based greatly upon the interval *between* impressions. For were the apparently greater rapidity of the light-succession due to its blur and after-image filling in the interval between impressions, and so making them appear to come closer together, then the 75 per cent. ratio of light to darkness should greatly increase the rate of the light succession, and the 25 per cent. ratio should decrease its apparent rate in somewhat the same degree. This, however, did not happen in the above experiments.

F. Throughout the previous work, a record had been kept of the natural and spontaneous manner in which the subjects formed their judgments. Four different ways predominated. Some said that they got an idea of the succession of the beats in the first series given, and noticed the likeness or difference in the rate of the second series to it. This mode will be designated by the term "general impression." Others seem to have judged by various synchronous muscular rhythms, of which I shall count as the second group those in whom this rhythm was less voluntary and conventional, occurring, *e. g.*, in the forehead, or in the back of the head. In a third group are placed those who counted; and in a fourth group, those who tapped.

Dividing twelve of the people used in the first part of the work according to their mode of making a judgment, three fall in Group I., three in Group II., four in group III., and two in Group IV. The percentage of respective judgments for the varying groups is as follows, when light rate and sound rate were physically equal (154 per sec.).

Group	L. F.	E.	L. S.
I.	73.6	25.6	.8
II.	20.0	70.8	9.2
III.	21.9	70.0	8.1
IV.	38.75	51.25	10.0

These figures indicate that those who simply get an 'impression' give nearly three fourths of their judgments as 'light faster,' while those judging by muscular rhythm and

those who counted give as such less than a quarter of their judgments. In the latter two groups the number of 'equality' judgments is increased to more than one half the entire number, while the 'light slower' judgments are greater than in the first and the fourth group.

G. Ten subjects who had never been experimented upon previously were taken to make a further study of the direct effect upon the judgments themselves of deliberately following these different ways of forming their judgments. They were first given the sound-light series with the eight light-rates and the 154-beat-per-minute sound-rate. One judgment was passed upon each series. Then ten judgments were made in the series in which the sound and light successions were both 154 per minute; the first procedure of one judgment on each series with a varying light rate was again gone through. Nothing was said as to ways or means of judging, so that each subject employed the method most natural to him. The above process was then repeated twice with other definite methods of forming the judgments now imposed. If the original method used had been by 'general impression,' the person was asked to count for one series, and to beat for the next. Where the 'general impression' method had to be imposed, to be certain that there would be no muscular rhythm, the subjects were asked to say and repeat, 'There is a black cat,' aloud as fast as possible. Great care was taken that this was in no way made to fit in with the rhythm of either series. This procedure after it had been tried one or two times by the subject in no way hindered the person from giving attention to the series and did stop any involuntary vocal synchronisms, so that the person would be judging by his impression alone.

When left to do as they would, six of the subjects beat, two counted, and two judged by their impression of succession. Throughout this part of the work it was noticed that while each individual may attempt to judge in the same manner, many variations still remain. For instance, in beating, a person may move his whole arm, his hand entire, or use his finger only slightly. He may synchronize the sound and

keep beating that rate throughout the light series, forming his judgments of the latter by the way its flashes fall in with his beating, or he may change his tapping as the flashes change, or he may cease to tap entirely for the light. He may tap a decided rhythm, he may synchronize well for the sound and not for the light, or he may not be able to do so for either. In cases where the tapping changed with the oncoming of the flashes, it was noticed that in some cases this change in the tapping would be reflected in the answer, while in other cases the difference in tapping was no indication of what the judgment would be. Similar differences were noticed in the counting. If a person tapped with marked irregularity, his counting was of the same sort. With all this variety in these two aids to the formation of time judgments it could hardly be expected that harmonious results for different individuals who are beating and then counting could be obtained. The average percentages of judgments by the three modes employed are, however, as follows:

Light Series and Sound Series at Equal Rate (154 per Minute)

General Impression			Counting			Tapping		
L. F.	E.	L. S.	L. F.	E.	L. S.	L. F.	E.	L. S.
52.5%	32%	15.5%	59%	29%	12%	55.5%	28%	16.5%

The later tables that give the judgments in more detail show the great difference in the different individuals. The number of 'light slower' judgments seems marked when we remember that in the first group of persons examined 7.6 was the average per cent. of such judgments given. The apparent equality points for these observers were, as found by the gradation method:

General Impression	Counting	Tapping
141.8	142	146.6

As elsewhere, the judgments by this method do not coincide with those made on the equal rates. In the six cases where the natural method was to tap, the average percentages indicate a falling off of 'light faster' judgments for the imposed methods with an approximate increase of 50 per cent. in the 'light slower' judgments when judging by the General Impression method. Results:

EFFECT OF DEPARTING FROM NATURAL METHOD

Subjects	Natural Method			Imposed Methods					
	Tapping			General Impression			Counting		
	Judgments of								
	L. F.	E.	L. S.	L. F.	E.	L. S.	L. F.	E.	L. S.
1	100%	—	—	80%	10	10	100%	—	—
2	60	35	5	50	40	10	60	40	—
3	20	65	15	—	60	40	20	80	—
4	25	60	15	50	40	10	50	50	—
5	90	10	—	60	40	—	60	40	—
6	80	20	—	80	20	—	70	20	10
Average Per Cent.	62.5	31.67	5.83	53.33	35	11.67	60	38.33	1.67
	Counting			General Impression			Tapping		
7	80%	10	10	30	30%	40	50%	20	30
8	30	30	40	80	20	—	50	30	20
Average Per Cent.	55.0	20.0	25.0	55.0	25.0	20.0	50.0	25.0	25.0%
	General Impression			Tapping			Counting		
9	50%	40	10	60%	40	—	80%	20	—
10	65	—	35	20	—	80	40	—	60
	57.5	20.0	22.5	40.0	20.0	40.0	60.0	10.0	30

Those who naturally counted show individually a considerable change in result under the other methods, though when these results are combined there is but a slight tendency to quicken the light during the general impression and to quicken the sound while tapping.

When counting and tapping are the imposed methods the 'light slower' judgments are markedly increased with one of the subjects.

In only four cases does the natural method tend to give a greater number of equality judgments.

Two individual records in this group deserve special attention. Instead of judging of the succession of beats as did the other subjects, one of these judged of the succession of the intervals, so that in tapping and counting, instead of matching his beat to that of the metronome, he counted and tapped after its beat. His results are as follows:

Judgments for 20 S-L, with both sound and light at the equal rate of 154 per minute,

General Impression	Counting	Tapping
65% L.F. 35% L.S.	40% L.F. 60% L.S.	20% L.F. 80% L.S.

Equality point when sound was at 154, and the light rate varied (gradation method):

General Impression	Counting	Tapping
154	138	154

A contradiction in the results obtained by the two methods thus is evident. While this subject's original method was that of the general impression, he said after he had tried the others, that tapping was by far the surest.

The other person had a noticeably high 'equality point'; in fact he was the only person of the twenty-nine subjects experimented upon who ever gave as an apparently identical rate, a higher rate of light succession than of sound succession. His results read:

	General Impression	Counting	Tapping
Equality point by gradation method (16 judgments each)	160	150	154
	F. E. S.	F. E. S.	F. E. S.
Distribution of judgments per cent. when light-rate = sound-rate (154). (10 judgments each)	— 60 40	20 80 —	20 65 15

From these various modifications of the experiment we may perhaps be justified in drawing the following conclusions.

1. The commonly accepted statement that of equal times marked off by light and sounds the light-limited durations seem shorter than the sound-limited is by no means universally true, when applied to the apparent *rate of succession* of series of light and of sound impressions.

The experience that the light succession is *less rapid* than the sound succession comes not infrequently, and with some observers comes indeed more frequently than does the opposite experience, that the light succession is more rapid.

2. With persons who are practiced the impression that of two equal rates the light rate is the slower does not appear to be influenced directly by the amount of such practice. But when there is no practice at all, as in the class experiments, a greater number of persons take the light series to be slower than take it to be faster.

3. The apparent difference of rate between light succession

and sound succession does not seem to be directly connected with the persistence of the light effect upon the retina, which makes the blank lapse of time between the impressions less in the case of light than in that of sound. For in the first place, this could not explain those not infrequent cases where the light rate seems slower; and, secondly, the effect of artificially varying this blank lapse without changing the rate itself affects in no simple and direct way the apparent rate of the impressions. The change of the blank interval to an interval less than has become familiar seems more frequently to have an effect similar to a change in the opposite direction (*i. e.*, an increase of the blank interval); although different persons respond differently to this experiment, and differently at different times.

4. The *higher the rate* of the two kinds of succession here compared, the more pronounced becomes, with most observers, the illusion that the light series is the more rapid.

5. The *longer* the series of impressions to be compared, the more pronounced is the illusion that the light series is the more rapid.

6. The order in which the two series is given affects the result: the impression of greater rapidity in the light series comes, with most observers, more frequently when the light series *precedes* the sound series.

7. The method of forming the judgment of the relative speed of these successions differs greatly with different observers. Those who naturally incline to assist their judgments by counting or by noticing some hidden organic rhythm, in general have less frequently the sense of greater speed in the light series than have those who depend upon their 'general impression' (whatever that may mean) or upon an overt tapping by hand or finger. Yet the illusion of greater speed in the light does not seem to depend upon the presence or absence of mental aid from any noticeable organic rhythm, whether voluntary or involuntary.

XX. A MEMORY TEST WITH SCHOOL CHILDREN[1]

BY ARTHUR H. CHAMBERLAIN

The Problem Stated

With a view to determining the power of recall in school children, a series of tests were made. It was sought to ascertain the effect upon the power to recall when:

1. A number of objects are displayed (*a*) singly; (*b*) three together.

2. The objects chosen interest, we might suppose, particularly (*a*) the boy's mind; (*b*) the girl's mind.

3. The subjects tested are of different school grades.

4. The subjects tested are of different sex.

Objects Chosen

Fifteen objects were selected as follows: pocket knife, roll of string, marble, watch, key, flat-iron, threaded needle, thimble, scissors, doll, pencil, notebook, two-cent stamp, five-cent nickel, and match.

These objects fall into three groups: First, those that interest particularly the boys and are handled by them in their daily routine. These are the first five objects listed. Second, those that might be expected to interest the girls. These are the second five objects named. Third, those that are of equal interest to both sexes. These consitute the final five objects. All of the objects are found constantly in the child's environment. Choice was made of objects that were not too greatly different in size.

The Subjects

The subjects were chosen from the third, fifth and eighth grades, sixty from each grade, one hundred eighty pupils, all told. They were equally divided, thirty boys and thirty girls from each grade. They represented several different

[1] From the Psychological Laboratory of the University of California.

schools, in various sections of the city where the tests were made.

Apparatus Used

The apparatus for the tests was simple: A circular disc of wood, one-half inch thick and eighteen inches in diameter served as a stand upon which, near its outer edge, the objects were arranged at equal distances apart. A second disc of like thickness and twenty-four inches in diameter, had an opening whose size could be varied as desired. The larger disc was placed four inches above the smaller, which rested flat upon a desk or table. The upper disc was held in its horizontal position and made to revolve over the lower disc as a wheel revolves on an axle. This was accomplished by means of a rectangular block having either end cut to a step-cylinder and housed or shouldered into the inner faces of the two discs at their centers.

Arrangement of Objects

Those five objects supposed to be more familiar to the girls were placed in sequence—the threaded needle, flat-iron, thimble, doll and scissors. In the same way those objects pertaining chiefly to the boys were arranged in sequence—marble, knife, watch, key and roll of string.

Modes of Display

During the tests there were two modes of display. First, the objects were shown in such a way that, at all times, three were visible, while as each new object entered this group, one of the older members of it dropped out. Second, they were displayed singly. Any one subject was tested with only one mode of display.

Method of Procedure

Each subject was allowed one minute for observation of the fifteen objects, whatever was the mode of display. In the first test, the subject was placed directly in front of the cut-out sector and, as the upper disc revolved, the objects came into view, three at a time. The subject moved with

the disc, thus keeping directly in front of the opening. In the second test, the same order of arrangement was maintained, but the opening in the disc revealed only one object at a time. By allowing three seconds for each exposure, with one second interval, a total of one minute was given as in the other test.

In every instance note was made of the object at which the observation was begun. The immediate recall was tested at the close of the experiment by having the pupil name all the objects he could remember. A list of these objects was recorded by the operator in the order in which the pupil named them. The subject was also given a sheet of paper having a circle described upon it and corresponding to the disc. Upon this circle he was asked to locate the objects in the order in which they were placed on the disc. Any additions to the list of objects originally recalled were placed to the observer's credit. Record was then made of the total number of objects recalled by each pupil, out of a possible fifteen; of the order of recall, that is, the sequence in which the objects were named; and of the order preserved in placing the objects on the circle. An object is said to be in a '*correct*' position when it is located upon the circle in a position exactly corresponding to its original position on the disc. Two objects were said to be in a '*relatively*' correct position when they simply changed places in location,—were transposed. Or, if two objects in adjacent positions on the disc were given a corresponding location upon the circle, but not arranged properly as regards other objects in the group, the placing was said to be relatively correct. As only objects not included in the list of those correctly placed are included in the 'relatively correct' column, the average for the former is usually greatly in excess of the latter. Whenever the average for correct placing is relatively high, the average for relative placing is considerably lessened. There are twenty judgments when the objects are displayed singly and twenty when they are displayed three together.

The Results

Table I shows the results of the test in the three grades with the two methods of exposure.

These results are then analyzed in several tables that bring out: (1) The effect of the different methods of display without regard to grade, sex or arrangement of objects; (2) The effect of the particular grade of pupil upon total recall, correct and relative placing and the like; (3) The effect of sex upon total recall, the recall of boys' and girls' groups,[1] etc.

Table I

	Sex	Av. No. Recalled	Av. No. Correctly Placed	Av. No. Relatively Placed	Av. No. Recalled, Boys' Group	Av. No. Recalled, Girls' Group	M.V. of Total Recalled
Grade 3:							
One at a time.....	Boys	7.4	5.7	.7	2.9	2.7	1.17
	Girls	8.1	5.4	.9	2.6	3	1.5
	Both	7.75	5.55	.8	2.75	2.85	1.61
Three at a time....	Boys	9.1	6.4	.9	2.9	3.7	1.92
	Girls	8	5.5	.7	3.4	3	1.4
	Both	8.55	5.95	.8	3.15	3.35	1.66
Grade 5:							
One at a time.....	Boys	10.5	3	2.5	3.6	3.3	1.7
	Girls	9.2	2.3	3.4	2.8	3.3	1.04
	Both	9.85	2.65	2.95	3.2	3.3	1.37
Three at a time....	Boys	9.5	3.7	2.9	2.9	3.2	1.4
	Girls	9.5	2.4	3.7	3.3	3	1.1
	Both	9.5	3.05	3.3	3.1	3.1	1.25
Grade 8:							
One at a time.....	Boys	9.2	5.7	1.9	2.7	3.4	1.04
	Girls	9.6	7.1	.4	3.8	3.2	1.24
	Both	9.4	6.4	1.15	3.25	3.3	1.14
Three at a time....	Boys	10	6.4	2.3	3.1	3.4	1.4
	Girls	10.1	6.6	2.1	3.1	3.7	1.5
	Both	10.05	6.5	2.2	3.1	3.55	1.46

Table II. shows the collective results with the two methods of display.

Table II

Method of Display	Av. No. Recalled	Av. No. Correctly Placed	Av. No. Relatively Placed	Av. No. Recalled, Boys' Group	Av. No. Recalled, Girls' Group	M.V. of Total Recall
Singly............	9	4.87	1.633	3.07	3.15	1.37
Three at a time....	9.37	5.166	2.1	3.12	3.33	1.46

[1] The letters G. and B. will be used throughout as referring to the objects spoken of as the girls' and boys' group respectively.

The display of the objects three at a time gives an advantage in the average number of objects recalled, as well as in the average number correctly placed. When shown three together the relative placing also is better than when the objects are displayed singly. The tendency to recall the G. or B. group receives very slight advantage in any one method of display over another. The M.V. of total recall varies only slightly in the two methods.

Table III. shows comparative results in grades three, five and eight.

TABLE III

Grade	Av. No. Recalled	Av. No. Correctly Placed	Av. No. Relatively Placed	Av. No. Recalled, Boys' Group	Av. No. Recalled, Girls' Group	M.V. of Total Recall
3	8.15	5.75	.8	2.95	3.1	1.63
5	9.67	2.85	3.12	3.15	3.2	1.31
8	9.72	6.45	1.67	3.17	3.4	1.30

The pupils of the fifth grade show a marked superiority over those of the third grade in the average number recalled. The eighth grade is not greatly in advance of the fifth in this particular. In correct placing, the third grade is almost abreast of the eighth, while the fifth grade drops back to one half the showing made by the third. In relative placing, the fifth grade students far excel those of the third and are much superior to the eighth. The fifth and eighth grades show about equal power in recall of the *B* group, while the third grade makes nearly as good a showing. In every grade the average for recall of the *G* group is slightly better than that of the *B* group.

Table IV. shows the effect of sex in all grades.

TABLE IV

Sex	Av. No. Recalled	Av. No. Correctly Placed	Av. No. Relatively Placed	Av. No. Recalled, Boys' Group	Av. No. Recalled, Girls' Group	M.V. of Total Recall
Boys .	9.28	5.15	1.86	3.02	3.28	1.43
Girls .	9.08	4.88	1.86	3.16	3.2	1.30

Table V shows the relation of boys to girls in the different grades.

TABLE V

Grade	Sex	Av. No. Recalled	Av. No. Correctly Placed	Av. No. Relatively Placed	Av. No. Recalled, Boys' Group	Av. No. Recalled, Girls' Group	M.V. of Total Recall
3	Boys	8.25	6.05	.8	2.9	3.2	1.55
	Girls	8.05	5.45	.8	3	3	1.45
5	Boys	10	3.35	2.7	3.25	3.25	1.55
	Girls	9.35	2.35	3.55	3.05	3.15	1.07
8	Boys	9.6	6.05	2.1	2.9	3.4	1.22
	Girls	9.85	6.85	1.25	3.45	3.45	1.38

In the third and fifth grades, the boys have a slight advantage over the girls in the total number recalled and in the number correctly placed. The objects composing the *G* group are recalled somewhat better by each sex. The ratio between the recall by girls and boys of the *B* group and the *G* group is only slightly different from the ratio between girls and boys in the total recall.

CONCLUSIONS FROM THE STUDY

The results of the various experiments would seem to justify the following conclusions:

1. Recall is stronger when the objects are seen three at a time than when shown singly.

2. The average for total recall shows a considerable increase from the third to the fifth, with an almost negligible increase from the fifth to the eighth grades. This difference is emphasized when we consider that the age-difference between the fifth and eighth grade was approximately twice as great as that between the third and fifth. In other words, ability to memorize or to recall does not increase regularly with advance in age or experience.

3. The total average of recall for all grades and with all methods of exposure of objects shows the girls not to be superior to the boys. This is not in accord with the usual outcome of experiments in memory. No clear difference is discernible between the boys and the girls in the attraction exerted by the so-called boys' group and girls' group of objects; for both sexes the girls' group was slightly more attractive.

XXI. PRACTICE IN ASSOCIATING NUMBER-NAMES WITH NUMBER-SYMBOLS[1]

BY WARNER BROWN

In a recent study[2] the writer employed the difference between the time required to perceive and name a series of colors and the time required to read the same color-names when they are printed out in type as a typical instance of the general rule that it takes longer to perceive and name a simple object than to perceive and name a word. At that time reference was made to an experiment by Bourdon in which the time for calling out the number of points of light in a small group was said to be no greater than the time for naming the corresponding arabic numeral.[3] On the basis of this experiment Bourdon argues that the process of perceiving a small number of points as a number is no more complicated than the process of perceiving the symbol of the number. If this is correct it means that it is possible in this case to associate the name of an object with the object itself as quickly as with the symbol of the name and this would make it seem probable that the association processes in color-naming involve difficulties peculiar to themselves and are not typical of the general situation in which simple objects are perceived and named.

What follows is an attempt to discover whether number-naming is really a process which is free from the time-consuming difficulties of color-naming. A practice experiment was devised in which number-naming was subjected to the same analysis that was applied in the previous case to color-naming. The material for the experiment was all prepared with the typewriter. It consisted of four sheets, each con-

[1] Studies from the Psychological Laboratory of the University of California.

[2] This REVIEW, p. 45.

[3] *Rev. philos.*, Vol. 65, 1908, p. 426.

taining ten lines of ten items. The first sheet contained the type-written words *one*, *two*, *three*, and *four*. There were 25 words of each sort, arranged in irregular order with not more than 3 nor less than 2 of a kind to a line. Four different sets of sequences of words were used to prevent memorization of any particular sequence. The lines were separated by triple space and the words were separated from each other by the space of three letters. The second sheet contained the arabic numerals corresponding to the words of the first sheet so arranged that each symbol occupied a position, which was relatively the same as the center of the corresponding word in the sheet of words. In this way the eye-movement factor was kept as nearly constant as possible. The third sheet (known as 'dots') was made up of dots arranged to represent the numbers as follows: *one* was represented by a *period*; *two* by a *colon*; *three* by a colon with a period after it; *four* by two colons. The fourth sheet represented the numbers by an appropriate number of oblique strokes ('scores') made with the key used in printing fractions on the typewriter.

The time was measured which was required to read aloud the one hundred items on each of these sheets. The sheets were read one after the other in the order given above, and then all four were read again, so that the time stated for each sheet on each day of practice is the average of two records, the first and fifth, second and sixth, etc.[1]

The accompanying table gives the results of eleven days' practice with this material on the part of twenty-four students. None of these students knew the real purpose of the experiment. They were all encouraged in the supposition, which came naturally to all of them, that they would be able with practice to read the "dots" or "scores" as fast as the words or symbols.

[1] The experiment with colors referred to above, made it clear that there is no considerable difference in time between successive trials on the same day. The average time for naming 100 colors was found to be 42.3, 42.3, 43.0, and 42.8 seconds for 4 successive trials; the time for 4 successive trials of reading 100 words was 29.0, 30.4, 29.9, and 30.5 seconds. There can be no serious objection, therefore, to comparing the rate of reading one sheet with the rate of reading another sheet when their sequence is the same each time. It is understood, of course, that the order of the items on the sheet is different on successive sheets.

TABLE FOR ELEVEN DAYS OF PRACTICE ON THE PART OF 24 PERSONS, SHOWING THE AVERAGE TIME REQUIRED TO NAME 100 ITEMS PRESENTED AS WORDS, AS ARABIC NUMERALS, AS GROUPS OF DOTS, OR AS GROUPS OF SCORES; AND SHOWING FURTHER THE RATIO OBTAINED BY DIVIDING THE TIME FOR THE ARABIC NUMERALS INTO THE TIME FOR EACH OF THE OTHER PERFORMANCES.

Day	Word	Arabic	Dot	Score	Ratio of Arabic to:		
					Word	Dot	Score
1	30.1	27.8	39.6	36.0	1.08	1.42	1.29
2	28.9	26.5	35.9	34.2	1.09	1.35	1.29
3	27.6	25.8	34.4	33.2	1.07	1.33	1.29
4	27.1	25.0	34.7	32.5	1.08	1.39	1.30
5	26.6	25.0	34.0	32.4	1.06	1.36	1.29
6	26.5	24.7	33.0	31.7	1.07	1.34	1.28
7	26.3	24.3	32.3	30.5	1.08	1.33	1.25
8	25.7	24.2	31.8	30.9	1.06	1.32	1.27
9	25.5	23.8	31.9	31.0	1.07	1.34	1.30
10	25.1	23.6	31.2	30.4	1.07	1.32	1.29
11	25.0	23.4	30.9	29.9	1.07	1.32	1.28

The result shows that the time required to perceive and name the number of a small group of marks is longer than the time required to perceive and name the corresponding word or the arabic symbol of the number. In this respect the results agree perfectly with the experiment in naming colors, and support the general dictum that the naming of objects is slower than the naming of words.[1]

The same peculiar inhibitions appear in the reading of the "dots" or "scores" which are encountered in color-naming. Some persons are more troubled by the "dots" and others find more difficulty in the case of the "scores" but no one notes any considerable disturbance of this kind in the case of the words or arabic symbols.

This experiment agrees with color-naming in the essential point that the ratio between the time required to name an object and the time required to name its symbol resists the

[1] As a matter of fact Bourdon's statement that the dots are named as fast as the symbols does not seem to be fully supported even by his own figures. His interpretation of his data raised a doubt which the results of the present experiment tend to clear away. There is no striking conflict between the original data of the two experiments. The method of serial reactions which has been used in the present case undoubtedly tends to magnify the loss of time in the association process and might reasonably be expected to show a greater difference in time between the two processes than would be shown by Bourdon's method of single reactions. But it is not probable that the serial reactions would show a difference unless the single reactions also gave some difference and as a matter of fact Bourdon's reactions do give some difference.

action of practice. This seems to argue in both cases that previous practice is not the basis of the relative rapidity of the latter process. So, too, the fact that the process of naming the symbol is itself capable of a very material improvement through practice precludes our speaking of it as automatic in comparison with another process (the slower one of naming the object) which improves no faster. It does not appear probable that differences in previous practice have much to do with the relative speed of the two processes.

The present experiment confirms the inference drawn from one part of the color experiment that phonetic symbols such as letters do not seem to be responsible for the advantage in speed of one association process over the other. It might be thought that the sight of the different letters would guide the complex movements of the vocal organs in uttering the word, and so facilitate the reaction to a word, but it appears that the words with their phonetic symbols can not be read quite as fast as the arbitrary arabic symbols which contain no phonetic elements. This is true for nearly every individual person and in spite of the fact that the spacing of the words and figures on the page gave nearly normal conditions of eye-movement for the words and rather unusual conditions for the arabic symbols.

In conclusion it may be said that the causes of the delay of the association processes in naming a simple object remain as obscure as ever. But on the negative side it seems clear that the greater speed with which words are named does not depend upon an advantage in practice and does not depend upon the suggestiveness of the letters in the words.

XXII. INCIDENTAL MEMORY IN A GROUP OF PERSONS[1]

BY WARNER BROWN

The study which is reported below leads to the conclusion that the factors which make it easy or difficult for an individual to recall certain of the incidental observations of his past experience also tend to affect in the same way the collective memories of a large group of persons.

The material for the investigation was obtained by having the members of a large college class write down, in a limited time, the names of all of the advertisements which they could remember having seen recently in the street-cars. They also, after making out the list, wrote down answers to certain questions about the advertisements, but that has nothing to do with the present report. The experiment was performed twice. The first time 175 persons wrote lists. The lists contained in all 896 items and these items were found to include mentions of 215 different advertisements. Thus the average person recalled 5.1 advertisements, and the average advertisement was mentioned 4.2 times. Table I is arranged

TABLE I

The Number of Items in the List	The Number of Persons Giving a List of this Length	The Total Number of Items in the Lists of this Length	The Relative Frequency of Occurrence of Such Items
0	21	—	—
1	8	8	23.1
2	17	34	24.8
3	15	45	16.2
4	22	88	22.6
5	16	80	20.8
6	20	120	18.0
7	13	91	21.9
8	12	96	19.7
9	9	81	15.7
10	7	70	14.4
11	6	66	14.4
12	3	36	11.4
13	3	39	9.5
14	3	42	12.5

[1] From the Psychological Laboratory of the University of California.

according to the length of the lists and shows the number of persons who gave a list of each length from 0 to 14 items. Table II. shows, roughly, the number of mentions accorded to

TABLE II

The Number of Items Mentioned	The Number of Mentions Received by Each of These Items	The Average Position of These Items in the Lists	The Number of Times One of These Items is Found at the Head of a a List, Per Cent.	The Number of Times One of These Items is Found in the 8th or a Lower Position, Per Cent.
{ 53	{ 1	5.4	5.7	22.6
{ 52	{ 1	5.3	11.5	19.2
34	2	5.0	11.8	19.1
8	3	4.6	11.1	14.8
14	4 or 5	3.9	14.8	14.7
8	6 or 7	5.2	7.8	19.6
8	7 or 8	4.5	11.9	17.0
7	8 or 9	5.0	10.2	18.6
6	9, 10, 11	4.4	17.5	10.5
4	12–15	3.6	16.6	7.4
3	15–18	3.8	28.0	10.0
3	18–20	4.0	28.1	12.3
2	23, 30	.8	24.5	15.1
1	55	3.5	20.0	5.5
1	55	2.9	30.9	5.5
1	58	3.0	24.1	5.2

different advertisements. Thus 105, or nearly half of them, are mentioned by but one person, while three of them are mentioned more than 50 times each. These three 'best' advertisements (Arrow collars, Spearmint gum, and a local confectioner), receive almost one fifth of all the mentions.

In what has just been said we have before us the essential points upon which the investigation is to be based. Some of the advertisements are much better remembered than others; and at the same time some persons remember a larger number of advertisements than other persons do. Is it true then, that the advertisements which are remembered by the greatest number of persons are the ones which permit of the easiest recall on the part of those persons who can remember several? The answer is obtained by finding whether the advertisements which are forgotten by most persons are written down later than those which can be recalled by more people. The result shows (Table II.), that in the average, one of the 105 which are mentioned only once is not mentioned until five others have been mentioned. The most popular advertisement has

an average position of third. In other words the average person will write down an item which many other persons can remember, sooner than he will write down one which only a few can remember. Less than 9 per cent. of the 105 straggling items are found at the head of a list, while about 24 per cent. of all the mentions of the most popular one are found at the top of a list.

Table II. has been arranged to show the result when the whole number of mentions is broken up into 16 approximately equal groups of from 50 to 60 cases each, on the basis of the frequency with which the items were mentioned. The table shows the average position of the item in the list; the items most frequently mentioned stand ahead of the rarer items in the lists. It also shows for each advertisement or group of advertisements the proportion of its mentions which stand at the head of a list; those most frequently mentioned are more apt to be mentioned at the very start. The correlation of rank

$$\left(r = 1 - \frac{6\Sigma D^2}{n(n^2 - 1)}\right)$$

in order of mention, in the above table, with average position in the list is .85, and with proportion of leading mentions it is .76.

While conducting this investigation the writer labored under the impression that those persons who mentioned only a few items would be apt to have peculiar reasons for remembering the few advertisements which they could recall, and that they would mention many wild and eccentric examples. The analysis of the data shows that such can not be the case. The sporadic item very seldom occurs early in any list; it can only rarely occur in a short list. The last column of Table I. shows the relative frequency with which the items in lists of different lengths are mentioned. This is found by listing all of the items in all of the lists of a certain length and entering opposite that item the total number of times that it is mentioned in all of the lists, *i. e.*, the frequency of the item. The total amount of all of these frequencies is then divided by the total number of items concerned. The

results of this computation make it evident that the items which occur in a very short list are items which are more often remembered than the items which occur in longer lists. As the length of the list increases it includes more and more sporadic or infrequent items. The original data show that of the 42 items contained in the one-item or two-item lists three-quarters were mentioned by more than ten different people, while of the 42 items contained in the fourteen-item lists three quarters were advertisements receiving *less* than ten mentions and one quarter were rarities, mentioned by less than four persons. Turning to Table II., the last column, it is evident that the less frequently mentioned items are more apt than the others to find mention toward the end of a long list, in the eighth or a still lower position. The correlation between rarity of occurrence and low position in the list is .78.

The facts warrant the conclusion that the items in the short lists are not determined by individual or special conditions, but are simply the items which are most easily remembered. There seems to be good reason to believe that the same factors, whatever they are, which cause an advertisement, or other similar incidental impression, to be recalled early in the memory of one individual cause it to be recalled early in the memory of another, regardless of the number of items which may, or may not, follow after it. The difference between individuals in this respect seems to be a difference in the *number* of the items recalled, and not in the kind or identity of the items. Items, which, for any reason, are difficult to recall appear late in long lists and do not appear at all in short lists.

This conclusion is significant for experimental work in memory as it puts a new value upon the relative position of the items in the recalled series. Apparently the first items to be recalled are generally those which make a universal appeal; the special personal appeals are reported later, or not at all. Moreover there seems reason to believe that "poor" incidental memory involves, at least with this material, no other abnormality than poverty or "weakness."

So far as the actual advertisements which were used in

the investigation are concerned, it is important to note that some make a much more lasting impression than others. The difference can not be expressed by saying that one advertisement appeals to more persons than another; it must be stated as a difference in the strength of the appeal. The good advertisement makes an appeal so strong that it can not be forgotten; the poor advertisement is forgotten by all except those persons who can remember very weak impressions.

The results of this investigation are fully confirmed by a repetition of the experiment some five months later with another college class, among the members of which there were only a few who had taken part in the first experiment. As a result, perhaps, of the interest aroused by the first experiment, the average number of items per student rose from 5.1 to 9.9. The number failing to report any advertisements fell from 21 to 3 although the class was only a third smaller. There were no lists of one or two items presented. In spite of the larger number of items per student, the variety of the items increased to such an extent that the average number of mentions per advertisement only increased from 4.2 to 4.6. Under these changed conditions all of the conclusions of the first experiment were confirmed. The correlation between frequency of mention and high position in the list was found to be .76. Of the straggling single items only 7 per cent. were found at the head of a list, while 24 per cent. of the mentions of the most popular advertisement headed lists. The correlation between popularity and primacy was found to be .61. On the other hand the correlation between infrequency and a position somewhere lower than ninth on the list was found to be .76.

In conclusion it may be said that the items which appeal to the largest number of persons make the strongest appeal to most of those persons, and that those items which appeal to only a few make a weak appeal even to them.

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